THURSDAY, DECEMBER 18, 1890.

SHAKING THE FOUNDATIONS OF SCIENCE.

DURING the past twenty years there has been gathered together round the site of the 1862 International Exhibition a national collection of museums and schools for instructing the people in art, natural history, and in pure and applied science. For a time a senseless howl was raised that the whole thing was a mighty job to benefit a few, and the antagonism reached its climax when it was proposed to celebrate the year of the Queen's Jubilee by erecting the Imperial Institute on the "inaccessible" spot at which so many millions from all parts of London—nay, from all parts of Great Britain—had congregated to study fishes and food, microbes and music, inventions and instantaneous illuminations, the colonies and coloured fountains.

At length the more sensible of the critics began to realize that the supposed sinecurists at South Kensington were enthusiastic workers who, had they devoted their intellectual powers to the common occupation of enlarging their balance at the bankers instead of to the uncommon occupation of enlarging our knowledge of Nature, would have become capitalists whose vested interests would have been deemed more sacred than life or honour. As it is, the hosts of Mammon now threaten the domain of science in Exhibition Road, for it is actually proposed to make an underground railway, with trains running at frequent intervals, right under Prof. Rücker's laboratory, right under Prof. Norman Lockyer's observatory, and skirting the electrical research laboratories of the City and Guilds Technical College, called the Central Institution.

The English nation (for what concerns the development of our science concerns the nation at large) must abandon its oft-boasted claim of being a practical people. Germany will laugh us to scorn, France will hit us with an epigram, and Italy will view us with polite amazement. To the Royal College of Science come from all parts of our country science teachers to be taught; to the Central Institution come, in addition to English lads, graduates from colleges in Europe and America to learn how England teaches the application of science to industry. Shall they come to find a rumbling earthquake led from early morn to late at night close to the foundations of the very pillars that have been erected at considerable cost in order to secure for the instruments placed on them freedom from even the vibration caused by passing footsteps? On concrete foundations, some 13 feet below the level of the street, rest many pillars of 9 feet square, each quite separate from its neighbour and from the floor on which the students stand; and the upper portion of each pillar is stuffed with a thick cushion of wool, so that the instrument resting on it may give as unwavering a decision as does the Lord Chancellor on the woolsack.

And even this is not sufficient to secure all the quiet that Urania loves, for, just as earthquakes are foretold by the fluttering of the wings of birds, so the approach of a cart at the other end of Exhibition Road is foreshadowed, long before it can be heard, by the uneasy trembling of the very delicate galvanometer needles. Hence must Urania's attendants, the students engaged in research, worship at her shrine by night; and woe betide them if they happen to assemble when a "small and early" is held in Exhibition Road; for then must they cover their heads and mournfully depart, persecuted by the rolling brougham, heartbroken by the rumbling growler, and driven to despair by the rattling hansom. Even at the Cavendish Laboratory, in a quiet back street in the peaceful University town of Cambridge, Lord Rayleigh's most accurate work on the electric units had to be done at night; and Wheatstone's galvanometer magnetic needles at King's College followed the penny iron steamers during the day rather than the electric currents he was measuring.

Judge, then, what will be the effect of passing trains, even though they be drawn by no

> "Kittle of steäm,
> Huzzin' and maäzin' the blessed ground with the Divil's oän teäm,"

on the telescopes in Exhibition Road, which, besides other work, by long-exposure photographs have discovered for us new stars never yet seen in telescope by human eye; or on the galvanometers, whose ray of light aids all electrical industries by pointing out electric flaws in glass and porcelain which the microscope would mistakenly pronounce perfect.

England has just given the Rumford Medal to Prof. Hertz, of Bonn, for his splendid work on electro-magnetic radiation. Will England allow the short-sighted policy of Mammon—

> "Mammon the least erected spirit that fell from heaven"—

to prevent Prof. Boys continuing his work on the same subject at South Kensington? We honour Cavendish for weighing the earth: and now, when Boys has discovered how to hang bodies (not human) by quartz fibres, and so to weigh the earth with far greater accuracy than Cavendish could have done had his attracting balls been the size of houses, shall Boys be prevented from using his own apparatus? Shall the analysis of alternate-current waves at the Central Institution be interfered with by other waves set up by the new burrowing earthworm? We can hardly think so, when we remember how even a dear old soul has just received the news that an underground railway is to run close against our national science schools and laboratories. She, like Huxley's chum Gallio, cares for none of these things, but she had the sense to observe promptly, "Why, it will make all the things wriggle inside."

To study one set of vibrations in the laboratories in Exhibition Road, when another disturbing set is superimposed by the vibration of the building itself, will be like listening to a violin solo when a brass band is braying in the neighbourhood. A prince, the Duke of Edinburgh, renowned for his musical skill, and distinguished as a violin-player at the Albert Hall close by, might fitly take the lead in jealously preserving the possibility for scientific research, the importance of which his father would have been the first to recognize. Urania's voice is like Cordelia's, "ever soft, gentle, and low," and catch it the student never can if his beams of light when moving across his galvanometer and electrometer scales begin

> "To trip it as they go
> On the light fantastic toe."

Radio-micrometers, magnetometers, and every other meter the motion of whose index must be greatly magnified, will become simply museum specimens, illustrating the blindly commercial enterprise of Englishmen, who removed from Trafalgar Square the statue of Jenner, because he only showed us how to save countless lives in every country, and was not distinguished, like the people whose statues remain, by killing many to add to England's landed estate. The 3-foot telescope now being erected in Exhibition Road, will, of course, become useless when the earth-shaking train goes whirling underneath it. Why not leave it at once, like a broken monumental column, to mark the spot where lies buried many a noble hope?

But those who know, from past experience, how "great effects from little causes spring," already ask whether the scientific education of many, and the carrying on of scientific researches, such as have already been conducted in the laboratories in Exhibition Road, may not be worth more to the nation than any number of twopenny-ha'penny railway fares.

And others, whose only claim to be heard is the possession of that rarest of all senses—common-sense—ask, Can it be possible that the nation, after having spent such vast sums on the erection and equipment of these laboratories, will now permit them to be rendered well nigh useless?

To move the laboratories of applied science is as undesirable as it is now impossible. Lord Carlingford, at their opening, congratulated the nation that a site had been selected for them close to the Museums, and stated that the French had even just moved the École Centrale of Paris, so that it might be placed in close proximity to the Conservatoire des Arts et Métiers. It is not likely that we shall now reverse our policy, divorce our Schools from our Museums, and, in securing such a result, waste all that has already been spent. If a railway is needed between South Kensington and Tyburnia, let it go under Queen's Gate, and not "Round by the Serpentine under the trees," as sung Lord Algernon. The Muse utters a cry of distress, let us join with her in shouting—

"Procul, oh! procul, este profani."

THE PALÆOZOIC FISHES OF NORTH AMERICA.

The Palæozoic Fishes of North America. By John Strong Newberry. (Monograph of the United States Geological Survey, No. XVI., dated 1889, issued August 1890.) Pp. 228, Pls. liii. (Washington: Government Printing Office.)

NEARLY all of importance that has hitherto been written concerning the Palæozoic fishes of the United States has proceeded from the pen of Prof. J. S. Newberry, of Columbia College, New York, whose numerous contributions during the last thirty years are to be found both in the American journals and periodicals, and in the beautifully illustrated volumes of a few of the State Surveys. Some of these contributions form extensive memoirs and are accompanied by numerous figures; but many are scattered notices without adequate illustration, relating to subjects worthy of much more elaborate treatment. The newly organized United States Geological Survey thus did good service to palæichthyology when it undertook, some time ago, to publish an extended monograph summarizing the whole of Dr. Newberry's researches and bringing them up to date. The result of the undertaking is the fine quarto volume now before us, which not only deals with forms already described, but also makes known a large amount of valuable new material, which the author has assiduously collected from various sources, and, for the most part, added to the unique collection in the Columbia College Museum.

The monograph occupies 228 pages, printed in the Survey's usual excellent style; and there are 53 plates, of which, unfortunately, little complimentary can be said. Eight of the latter are inferior process-reproductions of plates that have already appeared elsewhere, and many of the others are so carelessly produced by the same photographic method, that they give a very imperfect idea of the fossils they are supposed to represent.

Dr. Newberry treats the subject in stratigraphical order, and deals almost exclusively with the fishes of the Devonian and Carboniferous periods. Two pages only are devoted to the recent discovery of Upper Silurian fishes by Prof. Claypole; and no reference whatever is made to the few interesting Palæozoic types briefly described some years ago by Prof. Cope, from the Permian of Texas. The information concerning Devonian and Carboniferous genera, however, is most copious, accompanied by numerous references to literature; and we can only regret that the author did not continue his researches further into taxonomy, thus arranging the forms of each great period in some definite zoological order.

The limits of the Devonian age adopted by Dr. Newberry are somewhat different from those accepted by most American authors, and we doubt whether the inclusion of the Chemung and Catskill groups in the Carboniferous series will be generally accepted by either American or European geologists. The fish-fauna of each of these groups is certainly not Carboniferous, as ordinarily understood. Dr. Newberry considers that the Devonian series in the United States consists of the Oriskany (Lower), Corniferous (Middle), and Hamilton (Upper) groups; and the only fish-remains worthy of description are those from the two latter horizons. The account of the Corniferous fishes occupies 24 pages, and comprises little that is new, being almost exclusively a verbatim reprint from the Ohio Survey Reports of 1873 and 1875. A plate, illustrating several of the bones of the problematical Ganoid *Onychodus*, is the most important addition; and the remarks on the armoured fish *Macropetalichthys* constitute the most serious error that modern researches ought by this time to have eradicated. The fishes of the Hamilton group include more novelties, among which may be specially noted the triturating dental pavement apparently of an Elasmobranch (*Goniodus*), in addition to a new type of ribbed Ichthyodorulite (*Heteracanthus*), resembling in shape the well-known Carboniferous *Physonemus*.

The Chemung, Catskill, and Waverly groups constitute the Lower Carboniferous in Dr. Newberry's classification; and the series of limestones that contain the same fish-fauna as the lowest beds of the British Mountain Lime-

stone are termed Middle Carboniferous. Indeed, looking at the subject from the point of view of physical geology, Dr. Newberry adopts a well-known principle, and also assigns the whole of the Carboniferous Limestone of Western Europe to the middle portion of the Carboniferous epoch, considering that this great division of time terminates with the upper limit of the Permian. The fishes of the Chemung and Catskill groups comprise, among others, the genera *Palædaphus* (*Heliodus*), *Phyllolepis*, *Bothriolepis*, *Onychodus*, *Holoptychius*, and *Glyptopomus*, all of which are essentially characteristic of the Old Red Sandstone and Devonian of Europe, and are never found in the lowest mechanical sediments of the Carboniferous system even in Scotland; while the remarkable new genus *Holonema* is more suggestive of a Devonian Coccostean than of any later type. The fishes of the Waverly group, on the other hand, agree in general character with those of the Calciferous Sandstone and Carboniferous Limestone series of Scotland, except in one particular—namely, the occurrence in certain American horizons of the remarkable gigantic "Placoderms," to which Dr. Newberry has given the names of *Dinichthys*, *Titanichthys*, *Trachosteus*, *Glyptaspis*, &c.

The detailed descriptions of these genera constitute the most important part of the present volume, and add most materially to existing knowledge of the subject. *Dinichthys* is essentially a huge *Coccosteus*, often with jaws two feet in length. *Titanichthys* is a still more gigantic fish, the head sometimes measuring four feet across, and the long, slender, toothless jaws were apparently provided during life with a horny sheath. Some of the remaining genera are also noteworthy for the elaborate ornamentation of their armour; while at least one other (*Mylostoma*) has loose dental plates very suggestive of those of certain Chimæroids and Dipnoi. The series of *Dinichthys* and its allies in the Museum of Columbia College is, indeed, one of the most remarkable exhibitions to be seen in the institutions of the United States, and deserves the closest attention of all interested in the earliest types of fish life.

A contemporaneous Elasmobranch, occurring in the same formation as *Dinichthys*, is also a most striking addition to the Palæozoic fauna; but it is not described, and only forms the subject of two unsatisfactory plates. The dentition is Cladodont, and the species is named *Cladodus fyleri*, while the anterior half of an allied fish is both described and figured under the name of *Cladodus kepleri*. This form of Elasmobranch has small pectoral fins, with an abbreviated basipterygium, and the cartilaginous rays all extending to the outer border; the tail is attenuated, diphycercal, with a pair of horizontal dermal expansions at its base; there are apparently no dorsal fin-spines; and the orbit is surrounded by a complete ring of four ornamented dermal plates. The fish is altogether different from any hitherto found in Europe provided with *Cladodus*-shaped teeth, and increases the already considerable difficulties surrounding the classification of the predaceous Palæozoic sharks.

The discovery of an undoubted *Rhizodus* in the Carboniferous Limestone of Illinois is another fact of some interest; though in this case, as in several others, the author unfortunately omits to mention that the fossil has already been described, without figure, in the publications of the New York Academy. Several fossil teeth and spines of Elasmobranchs are also discussed from the same formation; and the final section of the work is a brief summary of the fishes of the American Coal-measures, accompanied by a reprint of the author's memoir on *Edestus*, which appeared in the Annals of the New York Academy in 1888. The last-mentioned fossil is still problematical, but is considered to be most nearly paralleled by the group of spines met with upon the tail of the existing *Trygon*.

Dr. Newberry's researches have excited so much interest among geologists residing in the neighbourhood of Palæozoic formations in the United States, that Columbia College is the destination of nearly all new discoveries of Palæozoic fishes from every quarter. So much has thus accumulated since the completion of the manuscript for the present monograph, that we understand the author contemplates the issue of an extensive supplement. That such an additional contribution may soon appear will be the wish of all naturalists interested in this line of research.

A. S. W.

HAND-BOOK FOR MECHANICAL ENGINEERS.

Hand-book for Mechanical Engineers. By Prof. Henry Adams, M.Inst.C.E. (London and New York: E. and F. N. Spon, 1890.)

THIS useful book is practically a new and improved edition of Prof. Adams's previous work, "Notes in Mechanical Engineering" The older work has been recast, and much additional matter added to render it of more use to mechanical engineers and to raise it above the level of the ordinary text book. The author rightly observes that busy men must have facts and opinions put before them as briefly as possible, and this is his reason for condensing the information in the book to a compact yet clear form. The work is sure to be an acceptable one to mechanical engineers. The information has in most cases been taken from trustworthy sources, and these are duly acknowledged. It is a pity that the index has not been differently arranged, for there is nothing more annoying when picking up a book of this kind in order to obtain some particular formula or information than to find that it is necessary to settle in one's mind what section it is likely to be in, and then have to search down the columns for a likely paragraph. Had the index been an alphabetical one pure and simple, the handiness of the book would have been greatly enhanced. In some few cases it will be observed that data are quoted from amateur engineering papers. These may be perfectly correct; at the same time the "busy man" will probably doubt their value and trustworthiness.

The book covers an extensive field of mechanical engineering, most branches being well treated. Some of the information, however, is antiquated; for instance on p. 27, we are told that wrought iron is used for "rails." We presume railway rails are here meant. It must be very many years since the last iron rails were rolled for that purpose, and it is possible that there are no works left in this country where the old iron plant is in existence. On the same page it is stated that Yorkshire iron from Lowmoor, Bowling and other forges is used for

tyres! This also is distinctly wrong. Who in the year 1890 would use iron tyres on railway rolling stock, when Yorkshire iron costs £20 or more a ton, and when steel tyres can be obtained for a fraction of the cost, without even considering the extra mileage obtained from the steel tyres? Prof. Adams tells us in paragraph 65 of the important process of casehardening wrought iron by heating it in contact with prussiate of potash, &c. In the opinion of many, ordinary bones used in the same way give far better results; and another way of obtaining the same result is to use wood charcoal, soda ash and a little lime. These give excellent results and are generally in use by manufacturers of locomotives and the like. Casehardening in many cases may extend to a depth of $\frac{3}{32}$ inch in important details of valve motions; pins and less important parts may do with a casing of $\frac{1}{16}$ inch in depth.

Paragraph 187 deals with cleaning castings, and here the author would put manufacturers "up to wrinkles," which they certainly are not ignorant of! He speaks of "Holes (we presume he means blow holes) stopped with black putty, cement, or lead." This is all very well in its way, but a casting requiring such treatment should be broken up and sent to the scrap heap. In paragraph 358 the author recommends that safety valves for boilers should have flat faces to prevent sticking. This is not the view taken by locomotive engineers. The most satisfactory valve is one with a conical seat fitted with three or four wings to keep it in position. The actual bearing of the valve on its seat should be about $\frac{1}{16}$ inch wide, or difficulty will be experienced in keeping it tight. In paragraph 369 new boilers are said to be tested to twice their working pressure in the best practice. The author must mean with hydraulic pressure, although he does not say so. This is certainly not the case with new locomotive boilers, nor is it to be recommended on any considerations. The bursting pressure required for any particular boiler can be approximately calculated near enough for all practical purposes, and with a suitable factor of safety allowed there is no necessity for this high test pressure on a boiler. To put a test pressure on a boiler much above its working pressure is to subject it to undue and unnecessary strains, and serves no useful purpose. The usual hydraulic test for a new locomotive boiler does not exceed 50 or 60 lbs. per square inch—about the working steam pressure. This is quite sufficient for the detection of bad workmanship, which is mainly the object of the test. The only satisfactory way to test a boiler is to have it periodically thoroughly inspected by a competent man. The mere testing a boiler with hydraulic pressure is no guarantee that the boiler is fit for work.

Paragraph 398, on the tractive force of a locomotive, is interesting. The reader is told that "the mean effective pressure on the piston is commonly assumed to be 85 per cent. of the boiler pressure." This wholly depends on circumstances. The speed of the engine and the cut-off have everything to do with the mean effective pressure. A heavy 6-coupled goods engine may occasionally exert a tractive force corresponding to a mean effective pressure of 85 per cent. of the working pressure, when starting a heavy train; but, as the speed increases, the cut-off is regulated by the driver and becomes earlier. With a working pressure of 150 pounds, with such an engine, and a cut-off of 30 per cent., the mean effective pressure becomes about 69 pounds per square inch, or about 46 per cent. of the working pressure. A mean effective pressure of 85 per cent. of the boiler pressure corresponds to a cut-off of approximately 70 per cent. of the stroke, or, in other words, the engine is in full gear. It is necessary to assume the highest possible mean effective pressure when the necessary weight on the driving or coupled wheels is being determined for adhesion, since the adhesive weight is in a fixed proportion to the maximum tractive force exerted; but for the purpose of determining the tractive force of a locomotive under varying conditions of speed and cut-off, the mean effective pressure of the steam in the cylinders will be found to be a varying quantity, as may be seen by studying the excellent paper on locomotives, read by the late Mr. William Stroudley before the Institution of Civil Engineers.

It is not necessary to say anything further on these points. The volume contains a mine of information. The theoretical portion is not unduly burdened with complicated formulæ. Those used are simple, and easily used by those not well up in the higher mathematics. Mechanical engineers have to thank Prof. Adams for putting within their reach a most useful book, clearly and concisely written, nicely printed and well bound, and one that certainly ought to be much used by mechanical engineers and those of the allied professions.

N. J. L.

FOSSIL FLORA OF AUSTRALIA.

On the Coal and Plant-bearing Beds of Palæozoic and Mesozoic Age in Eastern Australia and Tasmania; with Special Reference to the Fossil Flora. By B. O. Feistmantel. Memoirs of the Geological Survey of New South Wales, 1890. (Sydney: Charles Potter, Government Printer.)

THIS work is a translation from the author's older work in the "Palæontographica," amended to 1887, edited by R. Etheridge, Jun., Government Palæontologist, and with notes by the Geological Surveyor-in-charge. The greatest development of the older beds occurs in New South Wales, where the coals, sandstones, and shales with plant impressions, are intercalated with porphyries to the thickness of 14,000 feet. Two remarkable facts give special interest to the beds: one is that under beds with Conularia, Spirifer, and Productus, believed to be Upper Carboniferous, certain plants of Mesozoic type appear; and the other, that just under these plant beds there are conglomerates regarded as having been deposited by the action of ice. The formations in New South Wales, Queensland, Victoria, and Tasmania, are described separately, but together they form a series; commencing with the Devonian Goonoo Goonoo, containing Lepidodendron; the Lower Carboniferous Lepidodendron beds; the Boulder beds, showing signs of glacial action, correlated with the similar Dwyka Conglomerate of Africa, and the Talchir Boulder bed of India; the lower marine beds of Upper Carboniferous age, followed by coal measures with Glossopteris, and an upper marine series; the Permian Newcastle beds with Glossopteris and heterocercal fish; the Hawkesbury Trias with

Labyrinthodonts; and lastly some beds of Jurassic age. Until the base of the Upper Carboniferous is reached there is nothing abnormal, but at this point occur the remarkable intercalations of glaciated conglomerates, of which we have some slight indications in our English Permian, and which stretch both to India and Africa. This climatic change killed off the Lepidodendrons, and introduced a new flora containing the wide-spread Glossopteris and its ally Gangamopteris, and Nöggerathiopsis, types which would in Europe appear more at home in the Rhætic than in the Carboniferous, and which followed the changing climate as it spread to other continents. Meanwhile, the Carboniferous fauna of the seas is unaffected, and heterocercal fish accompany plants of quite Jurassic character in the Permian. This mingling of newer land floras with older marine faunas seems so universal in extra-European geology, particularly in America, that it appears as if during period after period the development of marine life was especially forced or quickened in the area which is now Europe, while terrestrial plant life was retarded, especially in England. The genera Glossopteris, Gangamopteris, and Nöggerathiopsis do not extend up beyond the Permian in Australia. In India Glossopteris is rare in the Jurassic, but its recorded extension into the Cretaceous of Russia must be questionable, while its supposed occurrence in the Tertiary of Novale is certainly due to its having been confused with the common Tertiary, and still existing, *Chrysodium aureum*, identical with it in venation but quite different in fruiting. In addition to these, the most noteworthy species are the beautiful adiantoid Rhacopteris of the Lower Carboniferous, to which several plates are devoted, and the Osmunda-like Thinnfeldia of the Mesozoic. The flora of the latter is only remarkable for its very European facies, and is said to include a Jurassic Sequoia, Cunninghamites, Baiera, Walchia, Taxites, and several Cycadeaceæ; it also shares with the Permian the fine *Brachyphyllum australe*, Feistm. There are altogether about 129 species or varieties described, of which about 50 are illustrated, and the volume is certainly a valuable contribution to the history of plant distribution.

J. S. GARDNER.

OUR BOOK SHELF.

The Development of Africa. By Arthur Silva White. (London: George Philip and Son, 1890.)

THERE can be no doubt that Africa is destined to occupy a much more prominent place in the thoughts of Europeans than it has occupied hitherto. The recent marking-off of vast "spheres of influence" may not have very important immediate results, but sooner or later attempts will certainly be made on a great scale to find out in these regions new channels for industry and commerce. It is evident, therefore, that the conditions of the development of Africa ought to be carefully studied; and in the present work, Mr. White supplies all the materials, so far as they are now known, for an adequate comprehension of the subject. He begins with a bird's-eye view of the continent, showing its geological and physical structure, its oceanic and inland drainage-basins, and the coincidence of political settlement with oceanic drainage-areas. Next he deals with the geographical distribution of the chief mountain-systems, and the consequent development of the great river-systems, in relation to accessibility from the sea and internal communications. Under the heading of "climate and cognate phenomena" he treats of the distribution of temperature; actual temperatures; the distribution of atmospheric pressure, and prevailing winds; annual rainfall; distribution of soils; zones of vegetation; distribution of animals; classification of climates; and acclimatization. Then come chapters on the indigenous populations, Islam and Christianity, the traffic in slaves, the progress of exploration, commercial resources, the European domination, and political partition. In the concluding chapter the author presents the general principles underlying the development of Africa along natural lines, derived from an examination of the various aspects under which the continent is known to Europe at the present day. He displays a thorough mastery of the facts relating to the various questions he discusses, and his work will be of genuine service to all who may be for any reason, whether theoretical or practical, interested in the utilization of Africa's material resources. The volume is accompanied by a most valuable series of maps, specially designed by Mr. Ravenstein.

The Pinks of Central Europe. By F. N. Williams, F.L.S. 66 pages, with 2 plates. (London: West, Newman, and Co., 1890.)

THIS forms a third instalment of Mr. Williams's studies of the genus Dianthus. His first paper was devoted to an enumeration and classification of all the known species. In his second he described the pinks of Western Europe, and traced out in detail their synonymy and geographical distribution. In the present paper he deals in a similar manner with the species that inhabit Central Europe. For Central Europe as a whole he claims 76 species out of a total of 230, which are disposed through the different countries as follows: viz. Austria, 59; Roumania, 24; Servia, 22; North Italy, 17; Switzerland, 15; Germany, 11; Poland, 7; Denmark, 5; and South Sweden, 4. His descriptions are clear and concise, but he has followed the Continental authors in classing as species many types which most English writers would place as varieties. None of the species are new, but it is a great convenience to have them all brought together, and treated on a uniform plan. The book is dedicated to Cardinal Haynald.

Materials for a Flora of the Malayan Peninsula. By Dr. G. King, F.R.S. No. 2. 93 pages. (Calcutta, 1890.)

THIS is a second part of Dr. King's Flora of the Malay peninsula, reprinted from the Journal of the Asiatic Society of Bengal. It covers the orders Bixineæ, Pittosporeæ, Polygaleæ, Portulaceæ, Hypericineæ, Guttiferæ, and Ternströmiaceæ. These were dealt with in Hooker's "Flora of British India" in 1872-74. Since that time a large amount of new material has been accumulated, principally by Kunstler, Scortechini, and other collectors whom Dr. King has sent out. Out of the orders just enumerated the first and two last are well represented in the Malay peninsula. In Bixineæ, Erythrospermum, known before only in Mauritius and Ceylon, is now recorded from Perak, an endemic species. In the same order the two curious Malayan genera, Teraktogenes, of Hasskarl, and Ryparosa of Blume, are added to the Indian flora, and four species of the former, and six of the latter, all endemic, are here for the first time described. In Polygaleæ there are ten new species of Xanthophyllum. In Garcinia, which has lately been fully dealt with in Pierre's "Forest Flora of Cochin China," fifteen new species are described out of a total of thirty-six now known in the Malay peninsula. In Calophyllum there are six new species, in Kayea four, and in Ternströmiaceæ two new species of Adinandra, one of Ternströmia, one Eurya, one Actinidia, two of Pyrenaria, and three of Gordonia. The publication of Hooker's "Flora Indica" has given

the Indian botanists a firm platform to work upon, and it is very gratifying to find that so much has been done within so short a time, and that the working out of the new material has fallen into such competent hands.

The Colonist's Medical Hand-book. By E. A. Barton. (London: Cassell and Co., 1890.)

THE author explains that this little volume has been "written expressly for the use of colonists and squatters, who are entirely out of reach of medical assistance." A more suitable book of the kind could not be at their disposal. They will readily understand his directions, and in recommending appliances for the treatment of emergencies he has taken care to refer only to such as are likely to be found in any squatter's hut.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Dr. Romanes on Physiological Selection.

AS Dr. Romanes now declares that the essence of his theory of physiological selection is "that some amount of infertility characterizes the distinct varieties which are in process of differentiation into species," and that the occurrence of infertility among the members of an undifferentiated species is secondary and comparatively unimportant, I ask leave to quote one or two more of his original statements, in addition to the four emphatic passages quoted in my communication of November 27.

(1) "When accidental variations of a non-useful kind occur in any of the other systems or parts of organisms, they are, as a rule, immediately extinguished by intercrossing. But whenever they happen to arise in the reproductive system in the way here suggested, they must inevitably tend to be preserved as new natural varieties, or incipient species. *At first the difference would only be in respect of the reproductive system; but eventually, on account of independent variation, other differences would supervene, and the new variety would take rank as a new species*" (NATURE, vol. xxxiv. p. 316).

The words I have italicized show clearly that variation in fertility only was what Dr. Romanes then claimed as essential to his theory. Again, after referring to variations in the season of flowering as a "well-known and frequently observed cause" of isolation, he adds:—

(2) "But it is on what may be called spontaneous variability of the reproductive system itself that I mainly rely for evidence of physiological selection" (*l.c.*, p. 337).

The meaning of this is still further enforced by other passages. After discussing the supposed causes of infertility, he says:—

(3) "Why should we suppose that, unlike all other such variations, it can never be independent, but must always be superinduced as a secondary result of changes taking place elsewhere? It appears to me that the only reason why evolutionists suppose this is because the particular variation in question happens to have as its result the origination of species" (*l.c.*, p. 339).

And again:—

(4) "It appears to me much the more rational view that the primary specific distinction is likewise, as a rule, the primordial distinction; and that *the cases where it has been superinduced by the secondary distinctions are comparatively few in number*" (*l.c.*).

Notwithstanding the passages I have now quoted, emphasizing eight times over, in different ways, that the theory is essentially one of variations as regards fertility and sterility alone, Dr. Romanes now says that, even if all this is wrong, "the principle of physiological selection, as I have stated it, is not thereby affected." If this is not an absolute change of front, words have no meaning; and it is further shown to be so by the fact that Dr. Romanes acknowledged that Mr. Catchpool had "very clearly put forward the theory of physiological selection." But Mr. Catchpool clearly distinguished between the old theory that species arise *first* by variation in form and structure, and only *gradually* become mutually infertile, and the new theory that they arise "by spontaneous variations in the generative elements, and are in this case *originally* mutually infertile, but only *gradually* become otherwise divergent" (*l.c.*, vol. xxxi. p. 4).

That this was the essential and original "physiological selection," that was claimed as supplying the missing link required to make the origin of species by natural selection a reality, is yet further shown by the repeated statements that physiological "selection" is a powerful preservative agent. Besides the statement already quoted, that variations in fertility "cannot escape the preserving agency of physiological selection," we have the assertion, quoted above, that such variations "must inevitably tend to be preserved as new natural varieties or incipient species," and the following still more emphatic assertion:—"Neither are we concerned with the degrees of sterility which the variation in question may in any particular case supply. For whether the degree of sterility with the parent form be originally great or small, the result of it will in the long run be the same: the only difference will be that in the latter case a greater number of generations would be required in order to separate the varietal from the parent form."

Now my contention has always been, and still is, that there is no principle at work which can accumulate or even preserve the variations of infertility occurring in an otherwise undifferentiated species, and that the term physiological "selection" is therefore a misnomer, and altogether misleading. If Dr. Romanes will carefully work out numerically (as I have attempted to do) a few cases showing the preservative and accumulative agency of pure physiological selection within an otherwise undifferentiated species, he will do more for his theory than volumes of general disquisition or any number of assertions that it *does* possess this power.

My next contention is, that this is the only new part of his theory—as he himself shows by his reference to the ordinary view, of sterility following other changes, as that which "evolutionists suppose." All the rest is to be found more or less fully discussed in Darwin's works; and I myself claim only to have carefully studied Darwin's facts, and his brief but most suggestive discussion of them in his chapter on "Hybridism" (vol. ii. of "Animals and Plants under Domestication"), and by arranging them more systematically to have shown that they do really give a fairly consistent and sufficient solution of the problem. The only part of my work I claim as a distinct addition to the theory is the proof that, under certain conditions that appear to me probable, natural selection *is* capable of increasing incipient infertility between distinct races or varieties; and the same view was submitted to Darwin twenty years ago.

Lastly, I totally and emphatically deny that any portion of my facts or conclusions on the subject were derived from Dr. Romanes's writings on "physiological selection." The only two sentences he has quoted from my book to prove that I have done so merely express what he himself has declared to be the common opinion of evolutionists, and which is also the direct outcome of the facts collected by Darwin. If this is "the whole essence of physiological selection," then physiological selection is but a re-statement and amplification of Darwin's own views, since he certainly assumed that "some amount of infertility" characterized "some varieties" of animals and plants, and that this infertility, when it occurs, is of some use in preventing the swamping effects of intercrossing. I feel sure that if *this* had been stated, at the outset, to be what was termed "physiological selection," no discussion would have arisen as to the principle involved, but only as to its novelty and as to the appropriateness of the name given to it.

If now, notwithstanding his repeated and emphatic statements that variation as regards fertility in otherwise undifferentiated species was what constituted the basis of his theory of physiological selection, Dr. Romanes continues to assert that I have adopted that theory "purely and simply, without any modification whatever," it will show that our respective standards of scientific reasoning and literary consistency are so entirely different as to render any further discussion of the subject on my part unnecessary and useless.

ALFRED R. WALLACE.

A Large and Brilliant Fire-ball Meteor.

ON Sunday night, December 14, between 9h. 40m., and 9h. 45m. G.M.T., I had the good fortune to witness the display of a most magnificent fire-ball meteor. It rose rapidly with a bright blue trail from an altitude of 6° above the horizon

at a point 7° south of west, and in about 7 seconds of time attained a culminating altitude of 55° at a point 19° north of west. Two large trees interlaced prevented me from seeing the head of the meteor till shortly before its culmination, but the light given out by it soon after its first appearance was equal to that of the full moon, and at culmination it much surpassed the light of a full moon in a cloudless sky. The ball seemed to be of a most dazzling bluish-grey colour, and it had a diameter of at least three-quarters of a degree. The disk presented a nebulous appearance with radiations within it as from a centre, but was well defined, except on its lower edge. The glare was almost too much for eyesight, and although the night was very frosty, calm, and clear, all the stars in the west became invisible. I turned to look again very shortly after, and at 5 seconds from culmination found the meteor had become a small yellow ball only one-twelfth of a degree in diameter, and was dropping ruddy sparks. It then disappeared at an altitude of 23° towards a point about 51° north of west. My impression was that this meteor was at no great distance from this place in any part of its course (lat. 51° 20′ N., long. 3m. os. E.). I noted the positions relatively to trees and tall shrubs, and measured them exactly with a theodolite this morning. To-day I hear that the fire-ball was seen to fall by a man in Chestnut Street, a hamlet on the Maidstone road, and then appeared quite close to him. The direction of fall accords fairly well with my own observation, and would make it descend about two and one-third miles from me. At culmination I should say it seemed very much nearer to me, considering especially its great apparent size at that time. I heard not the slightest noise either of rushing or bursting.

A. FREEMAN.

Murston Rectory, Sittingbourne, Kent, December 15.

ON Sunday, the 14th inst., about 9h. 45m. p.m., I was entering my house by the back door, when the whole place was so brilliantly illuminated that I momentarily supposed there had been a flash of lightning. That erroneous impression was at once removed by the continuance of the light. Wheeling round, I saw a splendid meteor of the fire-ball type, descending obliquely through the sky. Though the Monday newspapers reported serious fog in London on the previous day, yet the night sky at Loughton was perfectly clear; and it was easy to see that the meteor in its descent was passing a little to the right of the constellation Gemini, in a direction nearly, but not quite, parallel to a line joining Castor and Pollux. The head, which was downwards, was a large oval mass of light. The tail was not a mere thread of silvery radiance, like those of November 1866; it seemed broad, irregular on the edges, and sending out sparks. The fire-ball had not descended far when it vanished among a shower of sparks, which also very speedily disappeared. I heard no rushing sound during its course, and no noise of an explosion when it came to its end.

As the time available for observation extended to only a few seconds, it is possible that there may be some error of detail in the foregoing statements. I shall not, therefore, call them observations, but mental impressions of what took place.

ROBERT HUNTER.

Forest Retreat, Staples Road, Loughton, Essex, December 16.

Attractive Characters in Fungi.

IN the communications which have recently appeared in your journal on this subject, it has been taken for granted that, in the development of *spores* into *mycelium*, the former must necessarily pass through the body of an animal host. We have no scientific evidence of this. I am inclined to think that the theory is a remnant of the old superstition that toadstools are the result of the excrement of toads, and that we must seek for more natural processes of fertilization if we are to solve the mystery. Unbelief is sometimes the nearest road to a right faith. Scepticism is often the gate to truth. It is at least desirable that those who are investigating the subject should approach it unfettered by a theory which is yet destitute of proof, and should direct their researches to ground which is not littered with what may be only the fragments of an exploded superstition. My own observations tend to convince me that germination of the spore and development of the mycelium are alike dependent upon conditions of soil, modified by atmospheric influences, fertilizing agents, &c., &c. It may be added that the solution of the problem is rendered more difficult by the apparently inexplicable fact in Nature, so strikingly exemplified in the field of mycology,

> "that of fifty seeds
> She often brings but one to bear."

The countless millions of spores which never reproduce their kind must be transmuted into other conditions of being, to form part of the "living whole" of the universe, in which nothing can be lost.

J. S.

Glamis, December 5.

Some Habits of the Spider.

IT would be strange indeed, if, as your correspondents infer, there is no record of the gyrating habit of a species of geometric spider so common as even to be well known to Londoners who have a garden. It may be that Kingsley referred to this species, but his "Water Babies" is not found in scientific libraries. It occurred to me, last September, whilst amusing myself by making some of these spiders gyrate, by blowing on or gently touching them, that the instinct, in this kind, is in a decadent state; it does not appear well suited to a heavy-bodied, sharp-legged species like this one, and is certainly much less perfect than in other species possessing a similar habit.

Some years ago, when describing the habits of some Argentine spiders, I mentioned a species of *Pholcus*, abundant in La Plata, with legs of extraordinary length, in colour and general appearance something like a crane-fly, but double the size of that insect. When approached or disturbed in any way, it gathers its feet in the centre of the web, and swings itself round and round with the rapidity of a whirligig, so that it appears like a very slight mist on the web, and offers no point for an enemy to strike at. Here the correspondence between structure and habits is very perfect; the slimness and great length of the legs causing the creature, at the moment the swift revolutions begin, to seem to disappear from sight; and, owing to the string-like form of the legs, the fatigue experienced is probably very much less than the action would cause in a stout short-legged spider like the English species. At all events, it can revolve for fifteen or twenty seconds at a stretch; and, if the cause of alarm continues, it will perform the action no less than three times before quitting the web. The English spider exhausts itself in a few seconds.

Those of your readers who are interested in the habits of spiders will find the paper referred to in the *Gentleman's Magazine* for 1884. Some of my observations contained therein, I find, have been served up—after a fashion, and (of course) without acknowledgment—in a spider article in the current number of *Longmans' Magazine*.

W. H. HUDSON.

"Nowhere can Mathematics be learned as at Cambridge."

THESE words are ascribed to Dr. Hopkinson in his speech at the Royal Society on behalf of the medallists; and as they are calculated to sustain a belief which, Heaven knows, is already widely enough prevalent among even tolerably well-informed people in England—the belief, namely, that no one but a Cambridge Wrangler is worth thinking of as a teacher of mathematics—perhaps you will allow me to enter a protest.

In the department of applied mathematics, thanks to the work of Thomson, Tait, and Clerk Maxwell, Cambridge is supreme, and so far Dr. Hopkinson is right. The weakness of the Cambridge system has always been on the side of geometry; and I am sure that those who studied this subject under Hamilton, Salmon, and Townsend (to go no farther back), will agree with me in saying so. It is a weakness apparent in almost every work emanating from Cambridge. What would the subject of conic sections, for example, be, if Salmon had not shown how it should be treated? The answer is easily supplied by some of the Cambridge works on the subject which are still in extensive circulation in England. As a result of the belief which Dr. Hopkinson seeks to strengthen, the schools and Colleges throughout the whole country are becoming dominated by Cambridge methods exclusively, all appointments in them being virtually filled with Wranglers and no others; and hence we shall have, in time, a dead-level of sameness of method and thought which will partake of whatever shortcomings may characterize the Mathematical Tripos. Is this result desirable?

GEORGE M. MINCHIN.

R.I.E. College, Cooper's Hill, December 11.

THE DARKNESS OF LONDON AIR.

ONCE more the inhabitants of London are suffering from the inconvenience, that a darkened atmosphere naturally causes. This darkened atmosphere usually commences towards the end of October, and continues off and on sometimes to the end of February—that is, for a period of some sixteen weeks we partially and at times wholly deprive ourselves of the full benefit of the sun's rays, and live in a state of artificial darkness; and when we consider how very little daylight there is during the winter months (even under the best of conditions), it is surely very foolish to reduce the daylight, and create darkness in place of the daylight.

Is anything being done to remedy this state of affairs?

Apparently very little is being done to make matters better, whilst on the other hand a good deal is being done to make things worse; for instance, each year sees thousands of new buildings added to London, many of these buildings being factories and workshops.

We have now (within the Metropolitan area) some 765,000 tenements, and at present (with the exception of a few Acts dealing with factory smoke), the countless chimneys of these numerous buildings are permitted to belch forth volumes of sooty smoke, almost without any check whatever. As for the great majority of the buildings in London (that is, the dwelling-houses), we positively have no means of controlling their smoke.

Under these circumstances it is hardly very surprising, that the atmosphere over London in the winter is so unnaturally dark, and it is further not to be wondered at that we have to expend (or rather waste) vast sums of money in supplying ourselves with artificial light, to enable us to carry on our daily business. It is needless to point out that this outlay would be unnecessary, if we were really to adopt means to deal effectively with this smoke nuisance.

In NATURE, vol. xxxix. pp. 441 *et seq*, various results were given, which were intended to show, in an easily-understood manner, how dark the London air is during the winter-time, owing to smoke fog; it was also shown that other large towns suffer from the same cause.

It is now proposed to give some of the results of a few more observations, taken in London during the winter of 1889-90.

Table I. gives the results obtained in two districts in London, one being at the West End and the other at the East End, about 9 miles apart.

TABLE I.

District in London in which the chart was kept.	December 1889.	January 1890.	February 1890.	Total number of hours during which artificial light was used.
Hammersmith, W.	10	$3\frac{1}{2}$	3	$16\frac{1}{2}$ hours
Homerton, N.E.	68	38	$9\frac{1}{2}$	$115\frac{1}{2}$ hours

It will be seen that at Homerton the inhabitants lived for about $115\frac{1}{2}$ hours (during the three months named) in a partial state of darkness during the day-time, and if we consider how short the average length of each day is at this time of year, we find that out of the 90 days concerned, nearly 14 days were practically turned into nights, causing both inconvenience and expense.

During the month of December 1887, and the months of January and February 1888, the number of hours when artificial light was used at Homerton reached the total of $67\frac{1}{4}$ hours.

TABLE II.—*Chart for Black or Dark Yellow Fog.*

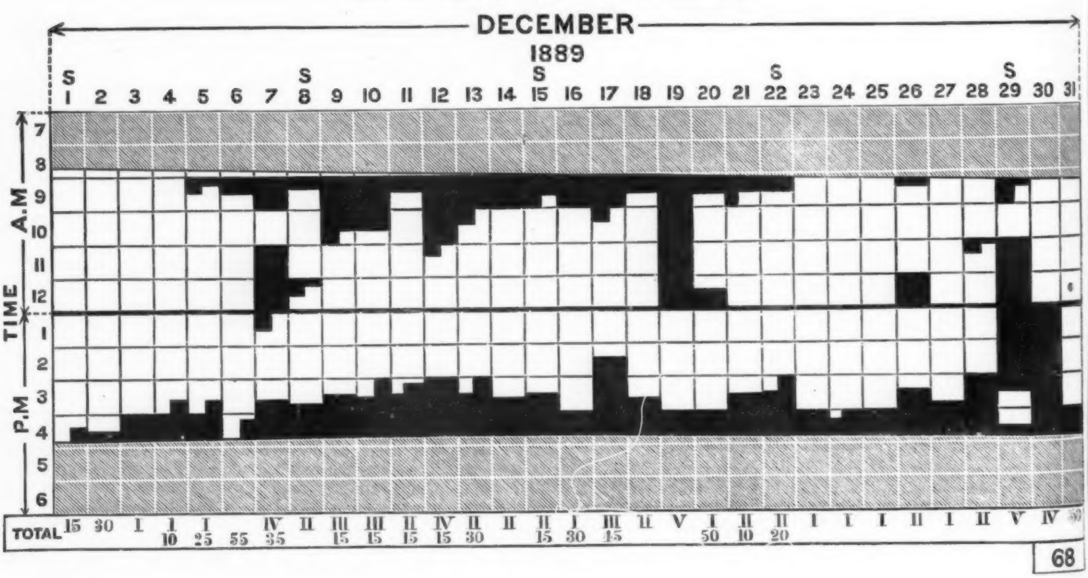

Table II. shows in detail, the kind of chart used to record the darkness caused by smoke fogs. I am indebted to a friend for partly suggesting the form of chart which is given in this table.

The original chart (only a portion being given here) was divided into 2160 squares, every 24 of these squares representing one day, and each single square one hour; these single squares are again subdivided into four parts of 15 minutes each.

The grey colour shows the night-time, the white the day-time, and the black denotes the time each day during which partial darkness prevailed owing to fog.

To show the serious way, in which this artificial darkness of the London air interferes with the numerous kinds of work, that depend upon a clear atmosphere and good light, various records were kept during the winter of 1889-90.

The results of two of these records are given below.

The photographer is, perhaps, one of the greatest sufferers from this artificial state of darkness during the winter-time.

There are various methods in use by which photographs are reproduced, and most of these methods depend on good natural light; Table III. deals with the "silver process."

TABLE III.—*Photography (Silver Process).*

	November 1889.		December 1889		January 1890.		February 1890.		March 1890.	
Number of days on which observations were taken in each month	26		24		27		24		26	
Number of days when no copy could be printed, owing to the darkness of atmosphere	7		10		7		3		5	
Longest time taken to print a single copy in one day[1]	5h. 10m.		5h. 45m.		4h. 40m.		4h. 45m.		3h. 0m.	
Shortest time taken to print a single copy in one day...	2h. 5m.		3h. 35m.		2h. 15m.		1h. 25m.		1h. 5m.	
	Days	Time taken	Days	Time taken	Days	Time taken	Days	Time taken	Days	Time taken
Average time taken to print one copy in so many days in each month	19	$3\frac{5}{19}$h.	14	$5\frac{3}{14}$h.	20	$3\frac{4}{5}$h.	21	$3\frac{1}{21}$h.	21	$1\frac{3}{7}$h.

[1] (*a*) In fine clear weather, it takes about 30 minutes to print a single copy. (*b*) Rain was the cause of no copy being printed on certain days; this remark applies to Table IV. as well.

These observations were taken at Ealing, London, W. It is hardly necessary to point out that Ealing is far away from the centre of London.

The record was kept daily (Sundays and Bank Holidays excepted) during the five months named in Table III.

It will be seen, that the shortest time taken to print a single copy on one of the 127 days in question, was about twice as long as it would have taken if the atmosphere had been clear, &c.; it will also be seen that, out of the 127 days on which observations were taken, on no less than 32 days no copy could be printed at all.

Table IV. deals with a method of copying drawings known as the ferro-prussiate process; this process is very extensively employed by architects, engineers, &c.; it absolutely depends upon a clear atmosphere.

TABLE IV.—*Ferro-Prussiate Process for Copying Drawings.*

	November 1889.		December 1889.		January 1890.		February 1890.		March 1890.	
Number of days on which observations were taken	21		15		15		17		22	
Number of days when no copy could be printed owing to the darkness of atmosphere	9		9		5		4		6	
Longest time taken to print a single copy in one day[1]	7h. 30m.		4h. 15m.		4h. 0m.		1h. 30m.		0h. 35m.	
Shortest time taken to print a single copy in one day	0h. 25m.		1h. 20m.		0h. 15m.		0h. 5m.		0h. 4m.	
	Days	Time taken	Days	Time taken	Days	Time taken	Days	Time taken	Days	Time taken
Average time taken to print one copy in so many days in each month	12	$2\frac{1}{6}$h.	6	3h.	10	$1\frac{4}{5}$h.	13	$\frac{8}{13}$h.	16	$\frac{1}{4}$h.

[1] In fine clear weather it takes about 4 minutes to print a single copy.

The observations were taken at New Cavendish Street, Portland Place, W.

Out of the 91 days on which a record was taken during the five months named in Table IV., on 33 days it was found impossible to print a single copy of a drawing, and on one day only could a copy be printed in 4 minutes, and this was on March 14, 1890; about 4 minutes is the shortest time possible, that a clear copy can be printed in.

It is, perhaps, unnecessary to give any more results in this paper, to help to prove the existing state of artificial darkness caused by these smoke fogs; but would it be asking too much, if we were to request the London County Council to take steps at once to enforce rigorously the existing Acts of Parliament, which have been framed to deal with factory smoke, and to consider (in a comprehensive and thorough manner) the best means of coping effectively with the smoke nuisance generally?

W. HARGREAVES RAFFLES.

VIBRATION-RECORDERS.

AN interesting paper, by Prof. John Milne, F.R.S., and Mr. John Macdonald, was read before the Institution of Civil Engineers on November 25. The authors presented an account of certain instruments which had been designed to register the oscillations and vibrations of trains, so as to give an automatic record of the run, and information as to the condition of the track. These instruments were modified forms of seismographs, the ordinary earthquake instruments being affected so much by the suddenness of the jolts, and by changes in inclination, as to be unsuitable for this purpose.

The apparatus for recording vertical movements consisted of a clock-spring coiled upon an axle, and connected with a lever carrying a weight in such a way that when the spring was wound the weight was supported by it. The result of this combination was, that for up and down movements of the apparatus some point in the weight remained at rest, and the relative motion of the apparatus and the weight was recorded by means of a pointer attached to the lever.

Horizontal vibrations were registered by the movement of pointers attached to two pendulums, the planes of motion of which were at right angles to each other. The simplest form of pendulum was a metallic cylinder, free to swing on pivots placed on its upper edge. The oscillations of the pendulum might be controlled by giving a certain frictional resistance to the movements of the pointers, as, for instance, by increasing the pressure upon the writing point. Another method of making the vibrations of the pendulum dead-beat, by coupling together two pendulums with a sliding joint, was also shown.

The recording-surface might be a drum, covered with metallic or ordinary paper, and driven by clockwork, the pointers being strips of metal, pencils, or pens; and to obtain a record extending over a considerable period a long band of paper rolled on a drum, as in the Gray-Milne seismograph, was pulled over another drum and wound up on a third, driven by its own system of clockwork.

Reference was made to fifty-eight locomotives now in Japan, on which experiments were carried out. In England, diagrams had been taken in carriages running on the London and North-Western, the Caledonian, the Great Northern, the South-Eastern, and the Metropolitan Railways; and in America a diagram, the original of which was exhibited, was taken, showing the motion of a carriage between New York and San Francisco, a distance of about 3300 miles.

As the diagrams produced by the vibration-recorder were evidently connected with the balancing of the engines, details of the weights of reciprocating parts and the balance-weights were given, together with the equivalent balance-weight, which was determined experimentally in the following manner. A pair of wheels, with their balance-weights, and crank-pins, were placed on the rails; a large round steel ring was then hung from the crank-pin, and from this weights were suspended until a balance was obtained. This weight hung upon the crank-pin was called the equivalent balance-weight, and it was found that when this weight was nearly equal to that of the reciprocating parts—that was to say, when the horizontal component of the energy due to the reciprocating parts was balanced—the diagrams for longitudinal motion were small.

Most of the diagrams were taken on locomotives running on the line between Tokyo and Yokohama, a distance of $17\frac{3}{4}$ miles, and they showed that there was a relationship between the character of the line and the recorded movements. Soft ground could be distinguished from hard ground, and the effects of bridges and culverts were recorded, all diagrams on the same line showing the same characteristics.

As an illustration of the use of the instrument, two diagrams were shown, taken on different engines running over the line from Shinbashi to Yokohama, the paper moving at the rate of 1 inch to the minute. It was pointed out that, as Shinagawa was approached, the diagram became larger. This was explained by the fact that this portion of the track rested on the mud of Tokyo Bay, and was, therefore, soft and yielding. At the third minute after starting, a sudden movement of the pointer occurred as the engine passed one of the culverts, but the cause of this jerk had not been determined. The train was seen to have left Shinagawa two minutes late, and to have stopped at Omori a minute and a half, instead of one minute, the schedule time. Between Omori and Kawasaki there were irregularities indicating soft places, but the most important mark occurred in the second span of the 40-feet girders forming the waterway leading up to the large bridge at Kawasaki. This mark was found regularly in all the diagrams, and a staff of workmen who knew nothing of the experiments having been sent to examine the place, reported that a sleeper on the second span of the down track yielded, when a train passed, twice as much as any of the other sleepers. Between Tsurumi and Kanagawa the diagram was rather larger, indicating a soft place that required attention. Yokohama was reached four minutes late, the places where time was lost being easily determined upon a close examination of the diagram.

The record of longitudinal motion was found to show most clearly differences in balancing, the size of the diagrams upon engines which differed only in the balancing having been as 6 to 27. A striking feature in the diagram of longitudinal motion was that it indicated ascents and descents by deviating to the right and left of a median line. Curves were marked in a similar manner by the portion of the instrument recording transverse motion.

The observation that certain locomotives gave a larger longitudinal diagram than others suggested that such engines were not exerting their power in an economical manner. It was found that the engines which had used the least coal per mile were those in which the difference between the weight of the reciprocating parts and the equivalent balance was small; and, on the contrary, in those engines where the difference was large the consumption of coal and oil was considerably greater. It occurred to the authors that engines yielding different diagrams might show a difference in the wear of their tires. The data obtainable were meagre, but they appeared to indicate that those engines which gave a small diagram for longitudinal motion did not wear out their tires so quickly as others.

The conclusions arrived at by the authors were:—(1) The vibration-recorder might be of value to those who had to deal with the management of the traffic, inasmuch as it furnished details of the times of stoppage and the speed at any part of the run. (2) The instrument might be used by those who had to inspect lines. Variations due to carelessness on the part of the plate-layers were recorded. Curves, ascents and descents, and even slight variations in grading, were indicated; faults in sleepers, irregular yieldings on bridges, soft portions of the track, and other imperfections, were definitely marked; and changes in the permanent way could be at once detected if such diagrams were taken at intervals. (3) The vibration-recorder might furnish information of value with regard to the manner in which a locomotive should be balanced, the vibrations due to this cause being measured by the diagrams for longitudinal and vertical motion. For this purpose, diagrams might be taken on a surface running at the rate of 1 inch per second. In this case each vibration of the locomotive was recorded separately, and from these diagrams the maximum acceleration of each backward and forward motion had been calculated.

GLACIAL CLIMATE.[1]

EVERY fragment of evidence, which can serve to show us the character of the climatal conditions during the last glacial period, is so important that I venture to present certain facts which, so far as I am aware, have hitherto escaped attention. The evidence I mean to discuss is found in America and Europe in the regions immediately south of the glaciated areas of the two continents. It is a well-known fact that in the present condition of the climates of the earth, the decrease in temperature as we rise above the sea is about 3° F. for each 1000 feet of altitude. Local circumstances may considerably affect this variation, but the range is not great. If glaciers were by the refrigeration of the climate restored to the surface which they occupied during the last ice period, we should expect to find the line of perpetual snow rising as we went southward about 3030 feet for each degree of latitude.

If, on an inspection of the areas glaciated during the recent ice epoch, we should find that this principle in the distribution of the glacier did not hold, we should apparently be justified in the supposition that the glacial climate was not due to greater cold than that which exists at present. Any departure from the normal rate of ascent of the perpetual snow-line in the region south of the glacier would be likely to throw some light on the climatal conditions prevailing during the time when the continental ice-sheets were developed.

Beginning our inquiry with the Appalachian section of Eastern America, we find there a region in many ways well suited for the determination we seek to make. The principal front of the ice stretched across the continent on a line which is now well determined. It is unmistakably evident that it crossed the valley of the Ohio at Cincinnati, and extended a little distance south of that stream into Kentucky. I have recently re-examined the evidence which goes to show the presence of the ice at the above-named point, and have no doubt as to the goodness of the determination. At this point the surface of the country lies at a height at no point exceeding 900 feet above the level of the sea. From this position the level of the country gradually rises in a southerly direction until in the synclinal mountains near Cumberland Gap it attains a height probably exceeding 3500 feet. From this elevation the profile descends in the broad valley of the Upper Tennessee to about 1000 feet above the sea-level; thence it again rises until, in the mountains of North Carolina, we enter a field where many peaks rise to more than 6000 feet in height. From the front of the ice-sheet near Cincinnati to the central part of the North Carolina mountain district is about 200 miles. It is to be observed that the whole of this district is within the same great valley, and in a region where the isotherms at the present time follow each other with normal curves. We may therefore fairly conclude that, under the usual conditions of climate such as prevailed in North America, the ice-line should be found in the mountains of North Carolina at the height of 2000 feet above the base of the glacier at Cincinnati, or, say, at 3000 feet above the level of the sea. From that level to the top of the North Carolina mountains, or, say, for the height of 3500 feet, we should have indications of glacial conditions. A tolerably careful investigation of this country has shown me no evidence of ice action whatsoever, and all the other students of the subject who have visited this area have failed to find any facts which might afford even a supposition of glacial work in that field. I am therefore compelled to assume that the slope of the snow-line rose so rapidly from the ice-front at Cincinnati southward that it passed above the summits of these mountains.

If the elevation of western North Carolina was in the form of an isolated peak, we might have less confidence in this indication. But the district of land which should have lain much above the snow-level is some thousand square miles in area, a field sufficiently great to have developed very extensive glacial areas in case the peaks lay above the line of perpetual snow. The same considerations, though in a less accented way, are met when we examine the highlands of the Blue Ridge in Virginia, or the Alleghany Mountain district on the uplands of Virginia and West Virginia. A large part of the Blue Ridge in Virgina is high enough to have been the seat of glaciers, provided the snow-line were anywhere near the level of the glacial sheet where it crossed the existing Atlantic coast. The traces of glacial work in the Blue Ridge are extremely scanty. At the western extremity of Rock Fish Gap, immediately south of the Chesapeke and Ohio Railway, near its junction with the Shenandoah Valley Railway, there are accumulations which apparently are to be classed as glacial. This point is about 1600 feet above the level of the sea. If this accumulation be really of glacial origin, it apparently establishes the height of the ice-front in the Shenandoah, but as yet I must regard the indication as somewhat questionable. In the Alleghany Mountains west of Covington, Va., there are deposits which I am disposed to consider of a glacial nature. At this point the deposits lie about 2000 feet above the sea-level. These are the southernmost points at which I have found any satisfactory indications of glacial work, in the region south of the Potomac, and, until further investigated, both of these deposits must be regarded as of doubtful character.

In Europe, in the region south of the Alps, we find the facts similar in their character to those existing in North America. During the last glacial period the ice-sheet extended down on to the Italian plains, unquestionably attaining levels less than 1000 feet of altitude above the level of the sea, and probably occupying positions not more than 500 feet above that level. From my observations on the field I am disposed to think that the general mantle of the ice covered the southern face of the Alps down to within a few hundred feet above the sea. From 150 to 200 miles south of the Alps in the mountains of Tuscany, we have an extensive surface rising 4000 or 5000 feet above the sea. A careful search over much of this field showed me no evidence of occupation by ice. At the present rate of rise in the perpetual snow-line in Switzerland we should expect an ascent of that plane about 1500 feet in passing from the foot of the Alps to the Apennine Mountains north of Florence. We have thus a case similar to that we find in the North Carolina mountains, in which there are elevations just south of the continental glaciers of a sufficient height to have been covered by ice under normal circumstances, but where the evidence of such coating is conspicuously wanting.

I have endeavoured to apply the same considerations to the glacial phenomena of the Rocky Mountains, but the facts are as yet so imperfectly in hand that I have not been able to determine the relative altitude of the sheet in a satisfactory manner. This, however, may be said: the distinct glacial accumulations in Colorado probably do not extend below the level of 6000 feet. As this region is about on the parallel of the mountains of western North Carolina, it may perhaps indicate that the snow-line lay throughout the southern parts of the United States above the summits of the Carolina mountains. It seems to me, however, that in the existing state of our knowledge of the distribution of the glacial sheet in the Cordilleran section we cannot attach much importance to this evidence.

We have now to consider the possible explanation of the facts above adduced. Assuming that the relative height of the surface occupied by the glacier, when it crossed the Ohio River, and that of the region within two hundred miles south of it, even during the ice epoch,

[1] By Prof. N. S. Shaler. Reprinted from the Proceedings of the Boston Society of Natural History, vol. xxiv. parts 3 and 4, May 1889–April 1890.

were what they are at the present day, it at first sight seems necessary to suppose that there was a rapid change in the temperature in passing from the ice-front towards the Gulf of Mexico. Before we adopt this consideration, however, we must bear in mind the fact that the ice-sheet of the last glacial period probably advanced for a considerable distance south of the perpetual snow-line, in substantially the same way in which an Alpine glacier descends in many cases to a depth of 1000 feet or more below the fields of enduring snow by which it is fed. Accepting the elevation of the continents as they now exist, and allowing 3° of temperature for each 1000 feet of altitude, it seems likely that the snow-line just touched the summit of the Carolina mountains and came to the surface of the sea near the southern end of Hudson's Bay. In other words, the protrusion of the ice to the south of this glacial snow-line carried it at a distance of near 1000 miles south of the gathering ground. This supposition, however, is of little value, for the reason that the level of the continent was clearly much disturbed during the glacial period, the surface declining to the northward within the glacial envelope, and probably rising to the southward of the ice-front.

It seems to me most likely that during the occupation of the northern part of the continent by glaciers, the southern portion of the continent was considerably elevated. All the streams which discharge into the ocean south of the former ice-front between New York and the Rio Grande show in their lower parts only moderate accumulations of alluvium which has been deposited since the close of the glacial period. They generally enter bays which appear to be the lower parts of gorges which were formed during the period when the area was more elevated than it is at the present time. These facts make it probable that if the mountains of North Carolina varied in elevation from the present height, they were more elevated than at this day. All the facts are against the supposition that we can explain the absence of glaciers in their highlands by supposing that the summits were lower during the ice period than they now are.

It seems to me we are compelled to suppose that the climate in the mountains of North Carolina, and probably in the great portion of the Apennine section south of the Alps, had during the glacial period a temperature not much if any lower than they have at the present time. As far as it goes, the evidence is thus opposed to the supposition that the glacial period was brought about by a general refrigeration in climate of the continents occupied by the sheet.

Within the basin of the Ohio, especially in the valleys of the Upper Tennessee system of waters, we find certain phenomena which lead us to the conclusion that the rainfall in a recent period, probably contemporaneous with the glacial epoch, was more considerable than at the present day. In many valleys which I have observed in that section the *débris* built into the imperfect alluvial plains is of a much coarser nature than that now brought down by the rivers. The channels bear the aspect of having recently been the seat of more voluminous streams than now occupy them. This evidence, gained from many points in the Southern Appalachians, leads me, independently of the hypothesis I am now suggesting, to the conclusion that during the last glacial epoch the rainfall of this country was much greater than it is at present. At Big Bone Lick in Kentucky, which lies within a few miles of the southern boundary of the ice-sheet, excavations made by me in 1868 show embedded in the deposits formed by the springs an abundant set of herbivorous mammals, including the mastodon and elephant, an extinct species of buffalo, and a musk-ox kindred to our Arctic species but of much larger size, a species of carabou, indistinguishable from our living American forms. The conditions of this deposit led me to suppose that these animals were probably not more ancient than the glacial period, and that they most likely occupied the surface during the time of abundant rainfall when the marshes were more extensive than at present, a period which if not exactly coincident with the extreme advance of the ice must fall within the glacial epoch.

The abundance of these large Herbivora, the relatively great size of the species, point also to the coincident occurrence of a rather abundant vegetation. If the period indicated by the massive gravels of the torrential streams and the Herbivora of Big Bone Lick be identical, and if this period coincides with the glacial period, as it appears to do, then we may fairly assume that the climatal conditions immediately to the south of the glacial sheet were not those of extreme cold. This evidence has nothing like the sure foundation obtained by the lack of glaciers in the mountains of North Carolina, but as far as it goes it confirms the results of those observations.

It is not my purpose, however, in the present writing to consider the perplexing question as to the cause of glacial climate. I desire only to call attention to the extent to which our glacial streams appear to have advanced, by what we may term forced marches, far to the south of the line of perpetual snow. Although the value of the evidence above noted cannot be determined until the matter has been more carefully brought together and abundantly discussed, the facts seem to me to militate against any hypothesis which seeks to account for the glacial period on the supposition that the climate in the glaciated regions was cooler than at present.

THE SUBJECT-MATTER OF EXACT THOUGHT.[1]

WHEN mathematicians, logicians, and other exact thinkers think and reason, they think and reason about something. What is that something? And wherein consists the infinite variety which it presents? Is it a mere assemblage of detached subjects of entirely different natures? Or is it an harmonious whole, admitting of a definition and treatment which, though perfectly general, will yet preserve the essential characteristics of its component parts?

To judge by the usual habit of thought on these questions, we ought apparently to conclude that the former is the correct hypothesis; but that such a conclusion would be wholly erroneous, there can, I think, be no doubt whatever. The prevailing view is due, I believe, to the want of a proper appreciation of the difference between that which is the essential or necessary matter of exact thought, and that which, so far as the processes of reasoning are concerned, is merely the dress in which that necessary matter is clothed. This dress has, of course, a real importance of its own, and the study of it is not to be undervalued. But when we are investigating the subject-matter of exact thought it is not with it that we are concerned: it is not with the case, but with the works of the clock that we have to do; and thus our anxiety should be to get rid of the environments, to treat them as the "disturbing agents" of the experiments of the physicist, as likely to mislead, and therefore to be eliminated with the most scrupulous care.

That such dress exists in the case of every subject that we investigate is obvious enough; much of it is recognized

[1] The object of this paper is to set forth in as simple and non-technical a manner as possible, the principles which were first formulated in my "Memoir on the Theory of Mathematical Form," published in the Philosophical Transactions of the Royal Society for 1886, vol. clxxvii. p. 1, and a "Note" thereon contained in the Proceedings of the Royal Society, vol. xlii. p. 193, which corrects the Memoir in some important particulars. The special applications of the theory principally dwelt on here are considered in detail in a recent paper by me, "On the Relation between the Geometrical Theory of Points and the Logical Theory of Classes," contained in the Proceedings of the London Mathematical Society, vol. xxi. p. 147. The mode of treatment of the whole subject adopted in the present communication must however be regarded as entirely new.—A. B. K.

without an effort. Thus the geometrician must draw the lines of his diagrams of some colour, but never dreams of supposing that the particular colour affects the real matter he is investigating; and the logician, bidding us fix our attention on the relations of implication, contradiction, &c., of the passages he selects for analysis, properly rejects the poetic beauties or philosophic truths they may enshrine as matters with which he is not for the moment concerned. But though there is a great deal of this accidental environment which is readily seen to be such, and is therefore rejected without difficulty, there remains much which, unless attention is expressly directed to its discovery and rejection, is likely to be regarded as the kernel itself, and not as mere husk. In innumerable instances this separation of the essential from the non-essential has, I am satisfied, not yet been effected. That this is so is, to a very considerable extent, due to the practice, no doubt for some purposes a necessary one, of dividing the study of the subject-matter of exact thought into different sciences, such as logic and mathematics, each with its own literature and students, the latter generally misinformed as to the real nature of those subjects which are not their special study. By this divorce of studies the student of either has the dress in which his particular subject is clothed so invariably associated with its essential elements that he fails to regard it as a dress at all, and looks upon it as a part of the naked body itself. No wonder then that such attempts as have been made to define the nature of the subject-matter of certain sciences are in general vague and metaphysical, rather than exact and mathematical; or that there are those who are content to say that logic is the science of "quality," mathematics of "quantity," and therefore their domains are, and should be kept, distinct. Such persons would no doubt be surprised to learn that mathematics is no more the science of quantity only than physiology is the science of the arm or leg; and that the algebra which expresses the relations of quantity is but a drop in the ocean of algebras which the mathematician can point to; another drop, be it observed, being the algebra which expresses the logical relations of classes or propositions to each other.

The essential elements of the subject-matter of exact thought are in reality of an extremely simple character; and, though they exhibit infinite variety, that variety is due to simple and easily defined causes. There is nothing vague or metaphysical about them; but, even when mere figments of the brain, they are precise and definite, with parts and properties which can be analyzed and catalogued, just as much as if they were the elements of a chemical compound, the wheels of a watch, or the organs of a vital structure. Let me try to show that this is so.

I will begin by considering and comparing the essential matter of two "branches of science," which will, I think, be regarded by most persons as of quite different characters, and as very properly relegated to separate and distinct treatises. I refer to the geometrical theory of points, and the logical theory of statements. The investigation will, I hope, fully prepare the way for an acceptance of the general definition of the subject-matter of exact thought which will follow.

I take points first. The geometrician deals, of course, with many things besides points, viz. lines, planes, curves, surfaces, &c.; but points may be considered alone, and it conduces to much greater clearness of ideas to confine ourselves to one species of entity, instead of introducing several into our field of view. I deal therefore with points only. And first let us consider individual points. Euclid says that they have neither parts nor magnitude; but these facts, however true and interesting, are, so far as the exact thought of the geometrician is concerned, beside the mark, and introduce considerations which tend to divert our attention from that which is really essential. When these immaterial considerations are swept on one side, all that we can say about points as individual entities is that they are all exactly like each other, are undistinguished from each other; no remark can be made of one which is not equally true of every other. But there are innumerable other entities about which the same thing can be said; and yet these do not possess those properties which characterize points. To say that points have *positions* is no explanation: it merely leads us to consider positions, and these are really the same things as points. Thus far, then, we have not obtained much insight into the essential characteristics of a system of points.

Let us go a step further and consider *pairs* of points. But even this will help us but little, for pairs of points are geometrically speaking, undistinguished from each other, just as single points are. No remark can be made about any one pair of points which is not equally applicable to every other pair. It is true, no doubt, that in some pairs the points are further apart than in others; but in considering distances we are not considering properties of individual pairs of points, but the relations of certain pairs to each other; and when we consider the relation of one pair of points, a, b, to another pair, c, d, we do not deal with peculiarities of the individual pairs a, b, and c, d, but with a peculiarity of the tetrad of points, a, b, c, d. To put the matter in another way, the geometrical properties of a diagram do not depend upon its size; whether it be enlarged or diminished they remain the same.

We are still, then, as much in the dark as ever as to what there is about a system of points, which gives to it the properties which are discussed by the geometrician; and we must advance yet another step, and consider *triads* of points. And here at last light breaks in upon us. Unlike single points and pairs of points, triads of points are not exactly like each other, and remarks may be made of certain triads of points, which, though true of some others, are not true of all. In the first place, there is the great division of triads of points into *collinear* triads, *i.e.* triads consisting of three points through which a straight line could be drawn, and *non-collinear* triads through which no such line could pass. This division is of fundamental importance, and the consideration of it introduces us to the whole of those geometrical properties which, for reasons into which I need not now enter, are termed "projective." But besides this division into collinear and non-collinear triads, there are apparently subdivisions of the triads of each sort, arising from the differences which may exist in the relative distances of the three points in a triad from each other—in the *shape* of the triangle which they form. The geometrical properties which depend on such subdivisions are known as "metrical" properties. We need not, however, separately consider these subdivisions; for, thanks to the researches of Poncelet and Cayley, we now know that the consideration of "projective" properties comprehends the study of those which are "metrical"; and thus a complete discussion of the results which flow from the division of triads of points into collinear and non-collinear triads, would include a full examination of those which arise from metrical subdivisions. In fact, when we consider the metrical properties of triads of points we are not considering triads at all, but collections of a larger number of points, arrived at by adding to the triads certain other points technically known as "the absolute." If we consider the triads alone and apart from "the absolute," *i.e.* if we consider their projective properties, we have only two sorts of triads to deal with, viz. collinear and non-collinear. All the triads of each sort are precisely alike in properties, nothing can be said of one of the triads of one sort which is not true of each of the others of that sort; but every collinear triad differs radically from every non-collinear triad.

All, therefore, that we need now concern ourselves with is the division of triads of points into collinear and non-collinear triads. Now, though the distinction between

collinear and non-collinear triads is of fundamental importance, it is not the collinearity and non-collinearity in themselves which are the subjects of consideration by the geometrician. The fact that in a collinear triad the three points are characterized by a symmetry or straightness which is not found in triads of the other species is, so far as the geometrician is concerned, a matter of indifference; it is a piece of accidental clothing which is not an essential part of the subject-matter with which he is dealing. He is concerned with the fact that triads of the one species differ radically from those of the other; but not with the fact that they differ in respect of straightness. With what, then, is he concerned beyond the mere fact that they differ? The answer is, that it is with the way in which the triads of the two species are distributed through the whole system of points; and with the fact that this distribution is not a random one, but is regulated by definite laws, so that if we are told that certain triads are collinear or non-collinear it necessarily follows that certain others are also collinear and non-collinear respectively.

The laws regulating this distribution may be stated as follows:—

LAW I.—If the two triads a, b, p, and c, d, p are collinear triads, there exists a point q such that the two triads a, c, q and b, d, q are collinear triads.

LAW II.—If the two triads a, b, c and b, c, d are collinear triads, so also are the triads a, c, d and a, b, d.

LAW III.—No point is absent from the system whose presence is consistent with the foregoing laws.

The distribution of the triads of the two species through the system in accordance with these laws completely determines it as one possessing all those properties which are usually studied by the geometrician. I insert the limitation "usually," because there are certain matters sometimes considered which require other circumstances than those previously referred to to be taken into account. To these I shall return later.

It necessarily, of course, follows from this definite distribution of the distinguished and undistinguished triads that there exists a similar regularity of distribution of distinguished and undistinguished collections of any number of points. But, in addition to this, there also exists a like regular distribution of what may be termed "aspects" of those collections, about which I must say something.

By an "aspect" of anything in the ordinary sense of the word, we mean the appearance which it presents under conditions. If the conditions are altered, we have different aspects of the thing. Thus, a thing presents different aspects when viewed from different standpoints, or when placed among different surrounding circumstances. The conditions may be purely mental. Thus, when we consider the relation of one of a collection of objects to the other objects of the collection, we mentally attach a different degree of prominence to the former object from that which we assign to the latter objects, and we have a particular aspect of the whole collection in view. An aspect of a collection of entities will also be obtained if we regard the entities of the collection as taken in a particular order. It is with aspects of this latter description that we are here concerned. It must not, however, be supposed that the *order*, in the sense of *succession*, is of any importance so far as we are here concerned. All that is material is the fact that the entities are conceived of as taken first, second, third, &c., and that to be taken first is different from being taken second, and so on, the nature of the difference being immaterial; so that an aspect of the sort considered would equally well be obtained by conceiving that each entity of the collection is marked with a different mark of any sort. It is, however, convenient for my purpose to consider the aspects obtained by supposing the entities of the collection to be taken in a particular order; among other reasons, because such aspects admit of being represented in an extremely simple manner; viz. if a, b, c, d... represent the entities of any collection of entities, we may represent the various aspects of that collection by arranging those letters in rows, thus *abcd*..., or *bdca*.... Aspects may be different, and yet be undistinguished from each other. Thus, by looking at a sphere from different points of view we obtain different aspects of it, but one of these may be exactly like another. So in the case of an aspect of a collection of any number of points; for example, to take the simplest case, of a pair of points, a, b. Here the two aspects *ab* and *ba* are different aspects of the pair, but they are undistinguishable from each other; nothing can be said of a pair of points taken in one order which is not true of the pair taken in the reverse order. In other words, a pair of points is *symmetrical*. On the other hand, if a collection of points is unsymmetrical, *e.g.* if it consists of a collinear triad, a, b, c, and one point, d, which is not collinear with a, b, c, then we can always find two aspects of the collection which are distinguished from each other, *e.g.* in the given case the aspects *abcd* and *abdc* are distinguished from each other.

The consideration of these aspects of collections of points is of much use in the discussion of the properties of a system of points. To take one simple example. Suppose we were told that the collection of four points, p, q, r, s, is undistinguished from the collection a, b, c, d, which we have just been considering. Then, though we know that three of the points p, q, r, s compose a collinear triad, we do not know which three; but if we are told that the aspects *abcd* and *prsq* are undistinguished from each other, then we know that, since a, b, c is a collinear triad, p, r, s is one also.

Every collection of points has, then, a number of aspects, arrived at by taking the points composing it in every possible order; and of these aspects of the various collections which compose a system of points, some are distinguished from each other, and some undistinguished; and these distinguished and undistinguished aspects are regularly distributed through the system, just as the distinguished and undistinguished collections are distributed. This regular distribution of the aspects is determined by the distribution of the collinear and non-collinear triads of points in accordance with the laws which I have given, and on that distribution alone.

Owing to this regular distribution of the distinguished and undistinguished collections, and aspects of collections, which compose a system of points, that system possesses a specific character or "form," as it may be termed. It is to the possession of this "form" that the system owes its various properties as studied by the geometrician, and it is this "form," and that only, which is the real subject-matter of his exact thought in the study of a system of points.

I do not propose to show here how the various properties of a system of points necessarily follow from the laws which I have given as defining its "form," or how it is that such a system contains sets of points having the characteristic properties of such as lie on straight lines, curves, surfaces, &c. These are all matters of detail involving no new questions of principle, and can be worked out without difficulty by anyone who has a knowledge of modern projective geometry. It is sufficient for my purpose to have indicated the position which "form" occupies in the geometrical theory of points.

Before, however, I pass away from the consideration of points, I must call attention to one or two matters with regard to them, which, as I have already indicated, may on occasion have to be considered by geometricians, and are not covered by the foregoing treatment.

I have hitherto assumed that the coincidence of points implies their identity, and in most geometrical investigations this is taken to be so. We may, however, regard coincidence as amounting merely to equivalence, *i.e.* we may regard coincident points as such that each bears

precisely the same relation to all other points.[1] Where a system of points thus includes equivalent points, pairs of points are of two sorts, viz. we have equivalent pairs and non-equivalent pairs. Equivalent pairs are distributed through the whole system of points in accordance with the following law:—

If each of the pairs *a*, *b* and *a*, *c* is an equivalent pair, then the pair *b*, *c* is an equivalent pair.

Another matter to which reference should be made is this. The laws which I have given as defining the "form" of a system of points define that form by specifying the mode of distribution of the collinear and non-collinear triads, *i.e.* of the various *collections* of three points to be found in the system. Nothing is said about the *aspects* of those triads, and thus we do not know, in the case of any collinear triad, which is the mean point of the triad, and which are the extremes—which point lies between the other two. In general it is not necessary that we should know this; in a vast number of geometrical investigations it is wholly immaterial. In certain cases, however, the matter is of importance; and, as its consideration brings out a very interesting and important fact as to the relation between points and statements, I must not pass it over. Here the laws which define the distribution of the collinear triads must not only specify what triads are collinear, but also which is the mean point and which the extremes in each. It must, however, be noticed that the fact that in a collinear triad one point "lies between" the other two is not a material circumstance; all that is of importance is that the one point bears a different relation to the triad from that which is borne by the other two points. It is convenient to denote a collinear triad in which *b* is the mean point, and *a* and *c* the extremes, by the symbol *ac.b*. The laws of distribution of the collinear triads may then be stated thus:—

LAW I.—If we have the collinear triads *ap.b* and *cp.d*, a point *q* exists such that we have the collinear triads *ad.q* and *bc.q*.

LAW II.—If we have the collinear triads *ab.p* and *cp.d*, a point *q* exists such that we have the collinear triads *aq.d* and *bc.q*.

LAW III.—If we have the collinear triad *ab.c*, and *a* and *b* are equivalent points, all the three points, *a*, *b*, *c*, are equivalent.

LAW IV.—If *a* and *b* are equivalent, we have the collinear triads *ac.b* and *bc.a*, whatever point *c* may be.

LAW V.—If the triads *a*, *b*, *c* and *b*, *c*, *d* are both collinear triads, so also are both the triads *a*, *c*, *d* and *a*, *b*, *d*.

LAW VI.—No point is absent from the system whose presence is consistent with the foregoing laws.

I shall have occasion again to refer to these laws, but, for the present, I pass away from the consideration of the geometrical theory of points, and proceed to discuss the logical theory of statements.

In place of points, we have now to deal with statements. Most persons would, I think, say that our new subject-matter is something altogether different from that with which we have hitherto been dealing, and would demur to my observation that, as subjects of exact thought, statements are just the mere entities that points are. "Statements," they would say, "are complex structures; some very complex, consisting, in fact, of a number of other simpler statements combined together by the use of the conjunctions 'and' and 'or'; and, of the simpler statements, even the most simple comprise parts—'terms,' &c.—which cannot be overlooked. It would be absurd to say that all these are to be ignored, and a statement regarded as if it were a mere entity such as a point is."

In answer to such objections, I would point out, on the one hand, that a statement is not other than a mere entity because it happens to be expressed as a combination of other statements, any more than a number is other than a mere number when it is expressed as the sum or product of other numbers. Nor, on the other hand, do we, by regarding a so-called "complex" statement as a mere entity, ignore the other statements in terms of which it is expressed, any more than we ignore certain numbers when we regard the number which is their sum or product as a single number, and not as a sum or product of two or more numbers. Those other statements will not be ignored, but will be regarded and treated each as a distinct entity. Every statement when considered alone is regarded as a single entity. When it is taken in conjunction with others, we see that it bears to them certain relations which we call "inconsistency," "implication," &c.; and, by virtue of the existence of these relations, it may be expressed in terms of those other statements, just as one number may be expressed as the sum or product of other numbers to which it is related.

As regards the "terms," &c., which compose statements, I remark that we shall here be concerned only with the relations of statements to each other, and not with the relations that they bear to "terms" or other things. The relations which statements bear to each other may, of course, be considered and expressed by dealing with the terms, &c., which compose them; and equally the relations of points to each other might be expressed by reference to the straight lines, curves, surfaces, &c., which pass through them: but, just as points have relations to each other which may be considered without reference to other geometrical conceptions, so statements and their mutual relations may, and will here be, discussed without any regard to terms.

But a further objection may be raised to the notion that statements as the subjects of exact thought are mere entities, such as points are, which must be considered and dealt with; and that is, that it leaves no place for those conceptions of the truth and falsity of statements which seem to be of the essence of the logical theory: ideas of truth and falsehood can hardly be associated with mere entities. In order to dispose of this objection, let us consider what the logical theory of statements is, as it is usually understood. Statements, with certain special exceptions to be presently referred to, are conceived of by the logician as admitting of being regarded at will as either true or false. This liberty, which we have to regard individual statements as either true or false, does not extend to all pairs, triads, &c., of those statements; for, in general, if certain statements are regarded as true, there are others which must be regarded as true also, and others which must be regarded as false. Similarly, if certain statements are regarded as false, there are others the truth or falsity of which is thereby determined. Our liberty, then, in this respect is subject to certain restrictions. It is to these restrictions that statements owe those mutual relations which it is the object of the logician to investigate and define.

I have said that there are certain special exceptions to the rule that individual statements are conceived of as admitting of being regarded at will as either true or false. The restriction, in fact, which exists in the case of pairs, &c., of statements, extends equally to the case of individual statements, for there are some which cannot be regarded at will as either true or false, but must be regarded some as always true, and others as always false. These statements are called *truisms* and *falsisms* respectively. Logically speaking, all truisms are equivalent: each bears precisely the same logical relation to every other state-

[1] Observe here the difference between undistinguishableness and equivalence. In order that two entities may be undistinguished, it is sufficient that the relation which one bears to any collection of entities may be borne by the other to a collection which is undistinguished from the former collection. But, in order that two entities may be equivalent, it is necessary that the relation which one bears to any collection of entities should be borne by the other to the *same* collection, and not merely to one which is undistinguished from it.

ment or body of statements; and this is equally the case with falsisms. The introduction of truisms and falsisms into our field of view brings us to the root of the difficulty about the truth and falsehood of statements, and enables us to dispose of it. For to regard a statement as true is merely to ignore the difference between it and a truism—to regard it as equivalent to a truism; and the conception of the truth of a statement is thus simply one as to the *equivalence* of two statements one of which is a truism. In the same way the conception of the falsity of a statement is one as to the equivalence of two statements one of which is a falsism; and generally the logical relation between statements which is expressed by saying that if certain statements are true or false certain others are also some true and some false, is one which may equally well be expressed by saying that if certain statements, one of which is a truism, or a falsism, are equivalent, so also are certain others, one of which is a truism, or a falsism. The truth of a truism and the falsity of a falsism are not matters with which we are here concerned at all, any more than we are with the elegance or conciseness of the language in which they are couched; the question is one merely as to the equivalence of statements, and the relations expressed are such as may and do exist between statements no one of which is a truism or falsism. In fact, truisms and falsisms, as regards the logical relations which they bear to other statements, differ in no material respects from any other statements; and indeed all statements are, so far as their logical relations are concerned, undistinguished from each other; for, whatever relation a statement bears to any body of statements, that relation is also borne by every other statement to some other body of statements.

How comes it, then, it may be asked, that truisms and falsisms unquestionably do appear to bear exceptional relations to other statements? The reply is simple. We are accustomed to consider statements with reference to the relations which they bear to truth and falsehood, *i.e.* to truisms and falsisms, and the verbal shape which they assume in general involves such a reference. In fact, as we shall presently see, whenever the words "*and*" or "*or*" are used, there is such a reference involved. Statements which thus involve a reference, whether express or implied, to the relations which they bear to truisms or falsisms naturally seem to bear exceptional relations to the latter; though in fact, logically speaking, they bear no such exceptional relations.

Statements, then, as subjects of the exact thought of the logician, compose a system of entities which are undistinguished from each other. Let us proceed, as in the case of points, to consider pairs of these entities. At first sight it may seem that pairs of statements are of many different sorts; for two statements may be equivalent, inconsistent, contradictory, one of them may imply the other, and so on. But, as regards certain of these relations, a little examination in the light of the preceding observations will make it clear that they are not really relations between *two* statements at all, but between *three*, one of which is a truism or a falsism. Thus two "inconsistent" statements are such that they and a truism cannot all three be regarded as simultaneously equivalent; and similarly in other cases, which will be considered when we come to deal with triads of statements. There are, in fact, but three species of pairs, viz. *equivalent* pairs, *contradictory* pairs, and *simple* pairs.

An *equivalent* pair consists of two statements which are such that, whatever logical relation one of them bears to any body of statements, that same relation is borne by the other to the same body. Equivalent pairs are distributed among the whole body of statements in accordance with the following law:—

If each of the pairs a, b and a, c is an equivalent pair, then the pair b, c is also an equivalent pair.

A *contradictory* pair, as usually defined, consists of two statements which cannot both be regarded as true or both as false, and the relation considered would therefore seem to be one between *four* statements, and not *two*. This is not, however, really the case. The relation may no doubt be thus defined by reference to the two additional statements—a truism and a falsism; but it may also be fully defined without reference to any such additional statements: viz. two statements are contradictory if they cannot be regarded as equivalent without ignoring all logical relations. Since two contradictory statements cannot be equivalent to each other, it of course necessarily follows that they cannot both be equivalent to a third statement, whether it be a truism or a falsism, or any other statement. Two statements which are a contradictory pair may be said to be *obverses* of each other. A truism and a falsism are obverses of each other.

The following law holds with regard to contradictory and equivalent pairs, viz.:—If a, b and a, c are both contradictory pairs, the pair b, c is an equivalent pair. As however this law is a necessary consequence of certain laws which we shall have presently to consider, I do not include it among the fundamental laws which define the properties of a system of statements.

The third sort of pair, viz. a *simple* pair, is one which is neither an equivalent pair nor a contradictory pair: *i.e.* it consists of two statements which are neither necessarily equivalent nor necessarily non-equivalent, but may at will be regarded either as equivalent or not.

The division of pairs of statements into the three species, and the distribution of the pairs of the different species in accordance with the foregoing laws is not enough to determine the properties of a system of statements; and we must, as in the case of points, go on to consider triads of statements. These are of two sorts, which, for reasons that will presently appear, I term "linear" and "non-linear" respectively. There are other subdivisions of the triads into different sorts, but the division into linear and non-linear triads determines the other subdivisions. As in the case of a collinear triad of points, a linear triad of statements consists of two statements which may be called the "extremes," and one which may be called the "mean." It is such that if the two extreme statements are regarded as equivalent the mean must also be regarded as equivalent to them, and in this respect also it resembles a collinear triad of points. Any three statements which are thus related compose a linear triad, and any three which are not so related compose a non-linear triad. I shall employ the same symbol to denote a linear triad of statements that I employed in the case of a collinear triad of points; viz. a linear triad in which a, b are the extremes and c is the mean statement will be denoted by $ab.c$.

These linear triads are not scattered at random through the whole body of statements, but are distributed in accordance with the following laws:—

LAW I.—If we have the linear triads $ap.b$ and $cp.d$, a statement q exists such that we have the linear triads $ad.q$ and $bc.q$.

LAW II.—If we have the linear triads $ab.p$ and $cp.d$, a statement q exists such that we have the linear triads $aq.d$ and $bc.q$.

LAW III.—If we have the linear triad $ab.c$, and a and b are equivalent statements, all the three statements a, b, c are equivalent.

LAW IV.—If a and b are equivalent, we have the linear triads $ac.b$ and $bc.a$, whatever statement c may be.

LAW V.—No statement is absent from the system whose presence is consistent with the foregoing laws.

The distribution through a system of statements of the triads of the two species in accordance with the foregoing laws completely defines the system as one possessing all those properties of statements which are really under consideration by the logician when studying the relations of statements to each other. The fact that the extreme state-

ments of a linear triad cannot be regarded as equivalent without also regarding the mean statement as equivalent to them is a necessary consequence of Law III.; and thus all that is essential in a system of statements, so far as the exact thought of the formal logician is concerned, is that it is a system of entities, pairs and triads of which are of different sorts, and are distributed through the system in accordance with the specified laws.

This uniform distribution of pairs and triads of statements involves of course a similar regularity of distribution of distinguished and undistinguished collections of larger numbers of statements, and also of the aspects of those collections; and consequently a system of statements possesses "form," and owes its properties to the possession of this "form," in precisely the same way as a system of points does. Thus in the case of the logical theory of statements, as in that of the geometrical theory of points, it is "form" as here defined which is the real subject-matter of exact thought.

A remarkable circumstance connected with the laws defining the "form" of a system of points, and that of a system of statements, will no doubt have already been noticed. If we exclude Law V. of the former set of laws, the two sets of laws are the same; and it is thus on this Law V., and that only, that the differences between the properties of the two systems depend. This Law V. is that which expresses the fact that two straight lines can only cut once; so that, if in geometry this restriction were removed, the study of points would in all that is essential be the same thing as the study of statements. I cannot pursue this very interesting fact any further here; but it will now be understood why the expression "linear" has been used with reference to certain triads of statements.

As the views here put forward as to the true nature of the subject-matter of the exact thought of the logician in his consideration of the mutual relations of statements are somewhat novel, I cannot well pass on with the observation that the rest is mere matter of detail, but must briefly allude to one or two matters of importance. And first let us consider those relations which are usually considered as relations between *two* statements, but are, as I have already said, really relations between *three* statements, one of which is a truism or a falsism.

In a linear triad, let the mean statement be a falsism, then the extremes cannot both be regarded as true. For to regard them both as true is to regard them as both equivalent to a truism, *i.e.* as equivalent to each other. But if they are equivalent they must be equivalent to the mean statement, which is a falsism, *i.e.* they must be false and not true. The two extremes are here, therefore, *contrary* or *inconsistent*.

If we take the mean statement of a linear triad to be a truism, then the extremes cannot both be regarded as false, they are *subcontrary*.

If we take one of the extremes to be a truism, then, if the other extreme is regarded as true, the mean must also be so regarded. Here the two latter statements are *subalterns*, the extreme being the *subalternant*, and the mean the *subalternate*. In common parlance the former statement "implies" the latter.

Next let me point to one instance in which certain relations of statements to each other involve others. If we have the linear triads $ab.c$ and $bc.d$, it can be shown to be an immediate consequence of the laws which I have given as defining the "form" of a system of statements, that we have also $ab.d$ and $ad.c$. Taking, then, b to be a truism, this becomes:—If the statement a implies the statement c, and the statement c implies the statement d, then the statement a implies the statement d, and $ad.c$ is a linear triad. Observe here that the last part of the conclusion is not usually pointed out, because the fundamental character of the linear triad has not been noticed.

I proceed finally to consider the use of the words "and" and "or," and the logical relation of statements such as "a and b," "a or b," to the statements a, b. If a, b, c be any three statements whatever, there exists a statement x, which is such that the three triads

$$ab.x,\ bc.x,\ ca.x$$

are all linear triads. This statement x is uniquely related to the triad a, b, c; that is, it is a necessary consequence of the laws defining the system that, if any other statement, y, is such that the triads

$$ab.y,\ bc.y,\ ca.y$$

are all linear triads, then the statements x and y are equivalent. This statement, x, I term the *symmetrical resultant* of the triad a, b, c.

Now let t be a truism, and f a falsism. Then the symmetrical resultant of the triad a, b, f is the statement usually written

"a and b,"

and the symmetrical resultant of the triad a, b, t is the statement usually written

"a or b,"

where, however, the "or" would be more properly written "$\frac{\text{and}}{\text{or}}$," as in mercantile documents.

In the general case, in which we take any three statements, a, b, c, without taking one of them to be necessarily either a truism or a falsism, the symmetrical resultant will be

"a and b, or b and c, or c and a,"

or

"a or b, and b or c, and c or a,"

the two statements being equivalent. These statements have tacit reference to a truism and a falsism, as they are expressed in terms of the statements "a and b," "a or b," &c., which are, as we have seen, symmetrical resultants of the triads a, b, t, and a, b, f, &c. But no truism or falsism is really involved in arriving at the symmetrical resultant of a, b, c; it is a function of a, b, and c, only. As, however, there is no simpler verbal expression for the symmetrical resultant in vogue, we are obliged to have recourse to one which expresses it by tacit reference to statements which are not really involved; just as, in the algebraic treatment of geometry, the relations of points to each other are expressed by means of an algebra which has tacit reference to an "origin" and "axes," which are not really involved in the relations which are represented.

I now pass away from the special consideration of statements.

The principles which I have enunciated in the case of points and statements may readily be shown to extend to entities of other descriptions. Thus, straight lines, curves, surfaces, &c., as individuals are mere entities, just as points are. Some are undistinguished from each other, and some are distinguished; thus one plane is exactly like another, but is distinguished from a straight line. These various entities owe their properties to the fact that they bear various relations to each other, and to points. The relations which they bear to each other are defined when we know those which they bear to points; and the relations which they bear to points are due simply to the fact that some points lie on them, and others do not. That a point does or does not "lie on" a curve is not, however, of itself a material circumstance; it is accidental clothing. All that is material to the geometrician is, that the pair of entities consisting of a curve and a point which lies on it is distinguished from the pair which consists of that curve and a point which does not lie on it, and that the two sorts of pairs are distributed through the whole system of points and curves in a definite way. Though the relations of these other geometrical

entities to each other may be arrived at by considering the relations which they bear to points, we may of course consider their relations without reference to points. Thus the system which consists wholly of straight lines is one composed of entities which are undistinguished from each other, and are such that pairs of those entities are of two sorts, viz. two straight lines cut or do not cut each other; and these two sorts of pairs are distributed through the system in accordance with definite laws.

Such considerations as these apply, not merely to straight lines, curves, surfaces, &c., but to all other entities which are dealt with by the geometrician. Vectors, quaternions, matrices, and even algebras, are as individuals all mere entities, and owe their properties to the fact that they bear certain relations to the entities previously referred to, and to each other; and all that is essential in these relations depends merely on the fact that certain individuals, pairs, triads, &c., and aspects of these, are distinguished from each other, and certain undistinguished, and have a specific distribution. Here therefore, also, it is "form" in the sense in which I have defined that word, and "form" only, which is under consideration.

In the same way terms, classes, syllogisms, and other logical conceptions are all mere entities; and all that is essential in their properties, so far as the exact thought of the formal logician is concerned, depends upon the fact that they compose systems possessing "form," *i.e.* a definite distribution of distinguished and undistinguished collections of entities, and of aspects of those collections.

A system of entities under consideration in any investigation will usually comprise entities of many sorts, some of which are taken into account merely for the purpose of facilitating the study of the properties of others. Algebras, and operations such as quaternions, are examples of entities thus added by the geometrician to a system of points, lines, curves, &c., in order to aid in arriving at the properties of the latter. By thus adding "auxiliary" entities to a system, we may also greatly simplify the definition of its "form"; so that a system which would otherwise be defined only by reference to the distribution of aspects, or of collections of a large number of entities, can be defined by definitions which refer merely to the distribution of collections of a small number of entities, and make no allusion to aspects. In fact, it may be shown that, by the addition of suitable entities, the "form" of any system of entities whatever may be defined by specifying merely that certain *individual* entities, and certain *pairs* of those entities, are like and certain unlike.

The light which is thrown by the foregoing investigations will, I hope, be sufficient to insure the appreciation, if not the acceptance, of the following general definition of the subject-matter of exact thought:—

Whatever may be the true nature of things, and of the conceptions which we have of them (into which points we are not here concerned to inquire), in the operations of exact thought they are dealt with as a number of separate entities.

Every entity is distinguished from certain entities, and (unless unique) is undistinguished from others. In like manner every collection of entities is distinguished from certain collections of entities, and (unless unique) is undistinguished from others; and every aspect of a collection of entities is distinguished from certain aspects of collections, and (unless unique) is undistinguished from others.

Every system of entities has a definite "form," due (1) to the number of its component entities, and (2) to the way in which the distinguished and undistinguished entities, collections of entities, and aspects of collections of entities, are distributed through the system.

The peculiarities and properties of a system of entities depend, so far as the processes of exact thought are concerned, upon the particular "form" it assumes, and are independent of anything else.

It may seem in some cases that other considerations are involved besides "form"; but it will be found on investigation that the introduction of such considerations involves also the introduction of fresh entities, and then we have merely to consider the "form" of the enlarged system.

If the definition of the subject-matter of exact thought which I have thus ventured to formulate be an accurate one, it will be obvious that there are divisions at present maintained between different branches of exact science which must be regarded as unnecessary and arbitrary; the only differences which should be treated as material being such as are due to differences of "form." These differences are certainly not such as to justify the isolation and separate study of individual systems, without any attempt to fix the position which each holds in the general body of possible systems, or to ascertain the exact points in which each resembles or differs from other systems of the whole body. The scientific study of each system must involve that of the properties common to all, of the general laws regulating the distribution of distinguished and undistinguished collections and aspects of collections in systems of any "form," and of the possibilities of variety in their "forms."

A. B. KEMPE.

NOTES.

THE retirement of Sir Gabriel Stokes from the Presidency of the Royal Society is to be taken as an occasion for the expression of the Fellows' high appreciation of his services. He has held office, either as one of the secretaries or as president, for thirty-six consecutive years; and it is unnecessary to say how much the Royal Society has benefited by his labours during that long period. It is thought that the most suitable way of marking the present occasion would be to obtain a good portrait of Sir Gabriel for the gallery of the Society; and an influential committee has been appointed to make the necessary arrangements.

AT a meeting of the City side of the Gresham Committee at Mercers' Hall on Monday, Mr. Karl Pearson, Professor of Mechanics and Applied Mathematics in University College, was elected Gresham Lecturer in Geometry, in succession to the Dean of Exeter.

DR. J. JAGOR, the eminent ethnographical traveller, intends to make a scientific tour in British India. Remembering his researches forty years ago, many men of science in Berlin take much interest in his present plans.

THE German Colonial Society have forwarded a number of books on tropical plants to Emin Pasha to further his scientific researches, as he lately complained (in a letter to Prof. Schweinfurth) of his want of works of reference. Prof. Noack has lately received a letter from the Pasha, dated Tabora, the middle of August. He then intended to leave for Uramba in four or five days, on his way to Lake Tanganyika.

AT a meeting of the Royal Botanic Society on Saturday, the Secretary answered various questions as to the destructive action of fogs on plants. He said it was most felt by those tropical plants in the Society's houses of which the natural habitat was one exposed to sunshine. Plants growing in forests or under tree shade did not so directly feel the want of light; but then, again, a London or town fog not only shaded the plants, but contained smoke, sulphur, and other deleterious agents, which were perhaps as deadly to vegetable vitality as absence of light. Soft, tender-leaved plants, and aquatics, such as the *Victoria regia*, suffered more from fog than any class of plants he knew.

ON Monday, Mr. T. G. Pinches read a paper before the Royal Asiatic Society, on the newly-discovered version of the story of the creation. He had had the good fortune, in the course of his investigations into the contents of the unregistered tablets in the British Museum, to find in one of them, brought home by Mr. Rassam in 1882, a still earlier version than that which the late Mr. George Smith had translated. It was a bilingual tablet, the text being Akkadian, and the gloss Assyrian; and while the date of the tablet itself was, like the rest of those in Assur-bani-pal's library, not older probably than 650 B.C., the Akkadian text was, in his opinion, an exact copy of an older document, which had, in all probability, been put into its present shape 3000 B.C., or even earlier. One side, the obverse, is devoted to the creation story. The other, the reverse, is simply an incantation form for the purification of the great temple tower E-zida, now so well known as the mound called Birs-Nimrud. The text might be roughly divided into three paragraphs or sections of about ten lines each. The first describes the time when nothing was, neither "the glorious house of the gods," nor plants, nor trees, nor cities, nor houses, no, not even the abyss (Hades) nor Eridu (regarded by the author as Paradise). The second section describes the making of Paradise with its temple tower E-Sagila, founded within the abyss. Then was Babylon made, and the gods, and the land, and the heavens, and mankind. The third section then proclaims the creation of animals, plants, and trees (in that order), of the Tigris and of the Euphrates. The fourth records the building of cities and houses. Of all except the last, Merodach, the god, seems to be the active creator, and he is also to be understood as the builder, through men, of the cities, &c. Mr. Pinches pointed out several interesting words and forms occurring in this oldest form of the creation account, which had subsequently assumed so many diverging shapes. A discussion followed, more especially on the word Adam, rendered by Mr. Pinches "foundations" (of earth), but by Dr. Zimmern "living things." This was probably the origin of the Hebrew word Adam.

ACCORDING to the *Journal de la Chambre de Commerce de Constantinople*, the greatest electric project which has yet been suggested is being planned—the construction of a line from St. Petersburg to Archangel. The electric current would be supplied by a series of generating stations distributed along the line. It is estimated that the cost, including the rolling stock, would be 46,509 francs per kilometre.

THE forty-first volume of the *Izvestia* of the Moscow Society of Friends of Natural Science contains a valuable review of the work done in zoology, anatomy, and embryology in connection with the Society during the last twenty-five years.

NATURAL History and Ethnographical Museums have been opened lately in several towns of East Siberia; and a like institution has now been established at Tobolsk. It contains most valuable ethnographical collections to illustrate the life of the Ostyaks and the Samoyedes, as also a very complete herbarium of the Tobolsk flora, and a collection of books, pamphlets, &c., on the region.

AT a recent meeting of the Paris Academy of Medicine, M. Motais, of Angers, maintained that myopia, or short-sightedness, is one of the products of civilization. An unexpected proof of this view was found in the condition of the eyes of wild beasts, such as tigers, lions, &c. M. Motais, having examined their eyes by means of the ophthalmoscope, discovered that animals captured after the age of 6 or 8 months are, and remain, hypermetropic, while those who are captured earlier, or, better still, are born in captivity, are myopic. This short-sightedness is evidently induced by artificial conditions of life.

M. A. BERTHOULE has contributed to the *Revue des Sciences Naturelles Appliquées* a series of papers, well illustrated, on the lakes of the Auvergne region, and their fauna, natural or introduced. He considers that new species might be placed in many of these lakes, and, if properly attended to, would yield large profits to pisciculturists.

THE curious idea of preserving dead bodies by a galvanoplastic method is not new, but we note that a Frenchman, Dr. Variot, has been lately giving his attention to it (*La Nature*). To facilitate adherence of the metallic deposit, he paints the skin with a concentrated solution of nitrate of silver, and reduces this with vapours of white phosphorus dissolved in sulphide of carbon, the skin being thus rendered dark and shiny. The body is then ready for the electric bath, which is served by a thermo-electric battery, giving a regular adherent deposit of copper if the current is properly regulated. With a layer of $\frac{1}{6}$ to $\frac{2}{3}$ mm. the envelope is solid enough to resist pressure or shock. Dr. Variot further incinerates the metallic mummy, leaving holes for the escape of gases. The corpse disappears, and a faithful image or statue remains.

MR. NATHANIEL WATERALL, Waddon, Croydon, writes to us that, in one of the outhouses in the garden belonging to the house in which he resides, a robin's nest was found some time ago in a flat hand-basket, hanging on the side of the wall. In the nest there were four young ones, recently hatched. They were allowed to remain until their time came to fly away. The basket had been in the same position for a considerable time.

PROF. E. A. KIRKPATRICK contributes to the *Evening Gazette* (Mass., U.S.), a paper in which he shows how parents —mothers especially—might do good service to psychology by recording "certain facts of child development." He suggests that they should devote particular attention to the growth of the power of speech. They ought, he thinks, to keep two lists of words—one containing all words articulated by the child, with indications as to how they are pronounced; the other, all words used "understandingly." The first list would indicate the common difficulties encountered in learning to articulate, and an examination of a sufficient number might make it possible to determine whether there really are "general laws of mispronunciation." The second list would show the intellectual progress of the child as it learns new words, and learns to use old ones with increasing accuracy, and to put them together into phrases and sentences. Words that are invented by the child, and those used in a sense different from the ordinary meaning, are especially interesting, and throw considerable light on the subject of how children classify and generalize. Prof. Kirkpatrick proposes that separate sheets shall be kept for each week, or perhaps for each month in the case of the articulating vocabulary. No confusion will then result, and on the back of the sheets may be given the peculiar meanings attached to words, the earlier attempts at putting words together, the later sentences of interest, especially those showing the characteristic grammatical errors, and other items. He is of opinion that the comparison of a number of such vocabularies would help to solve several interesting problems; and he expresses a hope that those who have begun, or think of beginning, the preparation of lists will send him the record for several months, before the middle of next May. Anything of scientific value that may be thus reached will be published, and along with it will be given "the names of those by whose patient observation it has been obtained."

PROF. BRÜCKNER, of Berne, has recently called attention to the existence of climatological periods of about 35 years for the whole globe (more marked in the interior of continents). The years 1700, 1740, 1780, 1815, 1850, and 1880 appear as centres of cold, wet periods; while the years 1720, 1760, 1795, 1830, and 1860 are centres of warm, dry periods. During the warm periods the passage of oceanic air to the continent has been hindered, and during the cold it has been favoured, increased rainfall occurring in the latter case.

It is stated that another meteorological station has just been added to the list of those in telegraphic communication with the Observatory at Si-ka-wei near Shanghai—namely, at Tientsin. M. Chapsal, whose services in promoting and improving the present system of semaphore signalling and meteorological observations are deserving of great praise, requested M. Ristelhueber, the French Consul-General at Tientsin, to use his influence with the Director-General of Chinese Telegraphs, to procure the free transmission of the necessary telegrams, with the result that permission has been obtained. The observations at Tientsin, made by Mr. Bellingham, will probably be of much value.

We have received a small pocket-book, published by Alfred Watkins, Hereford, on exposure notes, for use with the Watkins exposure meter. This meter seems to be of a very ingenious construction, and, according to the accounts we have read about it, should be found a very serviceable appendage to the photographic kit. The instrument is very neat and compact, being only $2\frac{1}{2}$ inches long and $1\frac{3}{4}$ inches in diameter, and consists of a combination of a bromide of silver actinometer, a chain pendulum for timing the exposure, and a set of calculating rings, each carrying a pointer. If each of these rings be set for the correct value of the factors indicated, a fifth pointer automatically indicates the correct time of exposure in seconds or in fractions of a second, as the case may be. In this note-book, full instructions are given as to the method of using it, followed by 60 pages in which notes on exposure, values of the factors, and other details may be inserted. In addition to the above, at the end are inserted various hints and jottings which should be found useful to the many photographers, both amateur and professional, who will use this note-book.

The Belgian Consul-General at Singapore, in a report quoted in the new number of the *Board of Trade Journal*, says that rubies and sapphires abound in the Siamese provinces of Chantaboun and Battambang. Several mines have been worked since a remote period by the natives, but for a long time they produced for the most part only stones of little value. It was in 1874 that the first mine of sapphires of good quality was discovered by a native huntsman in the environs of Chantaboun. The place was very difficult of access, so that the news of the discovery spread slowly. Rangoon being still at that time the nearest market to Siam for the sale of precious stones, the Burmans were the first to know of the existence of the new mine by the stones which were offered for sale at Rangoon. Some went there, and the large sums which they brought on their return from the sale of their produce brought about a movement of very active emigration for the same destination during the years 1878 and 1879. The new-comers discovered several mines as rich as the first. But there, as at Bantaphan, fevers made such sad ravages in the ranks ef the workers, that in 1880 the number of arrivals decreased in considerable proportions, and at the present time the population of these mines, which once reached the figure of 10,000, consists of a few Pegu Toung-Thons, who can ward off better than other races the ills resulting from the terrible climate of the country. Rubies, onyx, and jades are also found in considerable quantities in the province of Chantaboun, but their quality leaves much to be desired. Battambang is as rich in precious stones as Chantaboun, and it is stated that recently diamonds have been found near the frontier of Cambodia; but the mines of this province are almost abandoned because of the insalubrity of the climate, and the want of protection for foreign workers.

The canalization of the immense marshes of Pinsk, in West Russia, is rapidly going on. No less than 185 square miles of marshes have been drained on the banks of the Pripet, and more than 7,000,000 acres of meadow-land have been reclaimed in this way. Forests which formerly remained inaccessible and valueless, are now easy of access, and begin to be profitable.

A series of experiments upon the synthetical production of cyanogen compounds by the mutual action of charcoal, gaseous nitrogen, and alkaline oxides or carbonates, at high temperatures and under great pressure, are described by Prof. Hempel in the new number of the *Berichte*. Bunsen and Playfair long ago showed that when charcoal and potassium carbonate are heated to redness in an atmosphere of nitrogen, a certain quantity of cyanide of potassium is formed. Since that time Margueritte and Sourdeval have further shown that barium carbonate may be used in place of the potash, and that the barium cyanide produced may be again decomposed by steam into ammonia and barium carbonate. These reactions afforded a theoretically continuous process for the conversion of atmospheric nitrogen into ammonia, a process which, if it could only be worked on the large scale, would doubtless be of immense value. Unfortunately, however, only small proportions of the substances appear to enter into the reaction at ordinary pressures, hence the yield is not sufficiently large to render the process economical. Prof. Hempel, however, by means of a simple pressure apparatus, has shown that the reaction is very much more complete, and when potash is used very energetic, under the pressure of sixty atmospheres. His apparatus consists of a strong cylinder closed at one end, and worked out of a single block of steel. The steel top screws tightly down, so as to form a closed chamber, and is pierced with two apertures—one for connection with the compressing-pumps, and a second to admit the passage of an insulated copper rod. Within the steel cylinder is placed a smaller cylinder of porcelain, in which the mixture of the alkaline oxide or carbonate and charcoal is placed. Through the centre of this mixture passes a rod of charcoal, which is connected above with the copper rod and below with the steel cylinder itself, in such a manner that when the wires from a strong battery or dynamo are connected with the projecting end of the copper rod and the exterior of the steel cylinder respectively, the rod of charcoal becomes heated to redness. The pumps are then caused to force in nitrogen gas until the desired pressure is registered on the gauge. Experimenting in this manner it was found that the amount of barium cyanide formed in fifteen minutes under a pressure of sixty atmospheres was nearly four times that formed at ordinary atmospheric pressure; while in case of potassium carbonate the reaction was so energetic that in a few seconds the heated carbon rod itself was dissolved. Hence it is evident that the formation of cyanides by heating together alkaline carbonates and charcoal in an atmosphere of nitrogen is greatly accelerated by largely increasing the pressure under which the reaction occurs.

The additions to the Zoological Society's Gardens during the past week include a Cape Hyrax (*Hyrax capensis*), an Areolated Tortoise (*Homopus areolatus*), a Galeated Pentonyx (*Pelomedusa galeata*), two Rough-scaled Lizards (*Zonurus cordylus*), six Dwarf Chameleons (*Chamæleon pumilus*), two Rufescent Snakes (*Leptodira rufescens*), three Smooth-bellied Snakes (*Homolosoma lutrix*), a Rufous Snake (*Ablabes rufulus*), a Ring-hals Snake (*Sepedon hæmachates*), a Robben Island Snake (*Coronella phocarum*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; a Common Fox (*Canis vulpes* ♂), British, presented by Mr. C. T. Stanhope Bilbrough; a Demoiselle Crane (*Grus virgo*) from North Africa, presented by Mrs. Wright; two Cactus Conures (*Conurus cactorum*) from Brazil, presented by Mr. H. C. Martin; two Common Mynahs (*Acridotheres tristis*) from India, presented by Mr. G. W. Blathwayt; two Snow Buntings (*Plectrophanes nivalis*), three Bramblings (*Fringilla montifringilla*), British, presented by Mr. J. E. Baldwin; a Broad-fronted Crocodile (*Crocodilus frontatus*) from West Africa, received in exchange.

OUR ASTRONOMICAL COLUMN.

VARIATIONS OF CERTAIN STELLAR SPECTRA.—The November number of the *Monthly Notices of the Royal Astronomical Society* contains a note by the Rev. T. E. Espin, "On the Variation of the Spectra of R Coronæ and R Scuti, and on the Spectra of R Aurigæ and R Andromedæ." The following are the observations recorded :—

R Coronæ.

1890 March 26.—Star about 5·8 mag. Colour yellowish-white. Nothing certain seen ; sometimes irregularities, either dark or bright lines suspected.

1890 April 10.—Continuous spectrum, but again suspected lines ; one bright one strongly suspected near the place of F, but believed more refrangible.

1890 September 8.—A most wonderful change has taken place in this star's spectrum. Two large absorption bands have appeared, one in the bluish-green, and one in the bluish-violet. These bands are sharply defined on the least refrangible side. Bringing the spectrum to a line, bright patches were seen far away in the violet—these may be bright lines or bright spaces. The star is now pale yellow. The magnitude still about 6.

1890 September 14.—The spectrum is apparently of the IV. type [Group VI.] since the bands fade away on the more refrangible side, but are sharply defined on the less refrangible. The band in the bluish-green was thought to be occasionally resolved into fine lines ; between the two bands a bright line was suspected. The star is of the same magnitude, and now pale orange.

1890 October 8.—The star is only dim, but the bands seem to have faded.

1890 October 10.—The star has now nearly returned to the continuous first type spectrum observed in the spring. The big band in the bluish-green has disappeared, but the band in the violet is probably there still, but faint. The bright line previously mentioned again suspected. The star is now yellowish-white, and the magnitude about the same.

R Scuti.

1890 August 21.—III. Type (= Group II.). Bands 1, 2, 3 of Dunér's nomenclature seen, and also 7 and 8, which are the strongest.

1890 August 23.—Estimated 7·2 mag. ; pale orange-red ; carefully examined. Type III., but peculiar. The usual bands 1, 2, 3, seen ; the bands 4 and 5 faint ; 7 and 8 are strong. Bringing the spectrum to a line, bright knots seen in the violet and ultra-violet, either lines or spaces.

1890 September 8.—Bands 4 and 5 much better seen, but the other bands remain the same. The star is now brighter.

1890 October 10.—The star is now about 6 mag. The III. type spectrum is no longer certainly seen. A remnant of band 7 still remains ; the others have almost, if not quite, disappeared. Perhaps 8 is there.

1890 October 12.—The star about 6 mag. Type doubtful ; possibly III., but very indistinct.

1890 October 15.—The star is about 6·5. The spectrum is again clearly III. type. Bands 7 and 8 best seen ; also 1 to 4 visible. The bands are, however, faint and dim, but are larger now, and the spectrum is similar to that observed on August 21.

1890 November 1.—Normal III. type, and about 6·8 mag. The bands are well seen in all parts of the spectrum. Bands 7 and 8 are especially broad.

R Aurigæ.

1890 August 18.—Mag. 7·2 ; colour fine rose-red. Very fine III. type, and the spectrum appears to resemble that of Mira rather than that of R Andromedæ.

R Andromedæ.

1890 August 23.—Mag. 7·3 ±. Bands not deep except in the blue and violet. Bringing the spectrum to a line, several bright lines in the violet and ultra-violet suspected. F possibly bright.

1890 September 8.—The star has increased in light, and γ hydrogen and F certainly seen, but still faint.

1890 September 14.—Bands in the red well seen, the yellow bands faint. F very plain now.

1890 September 15.—The F line now a wonderful spectacle. The star is not so red, the bands are generally faint, except in the red. A bright space in the yellow looks like a mass of fine bright lines. A deep band in the violet—$H\gamma$ and D_3—possibly bright.

BRITISH ASTRONOMICAL ASSOCIATION.—The first number of the Journal of this Association has been recently issued. Miss A. M. Clerke has contributed a paper on the rotation periods of Mercury and Venus, in which she brings forward the evidence which led Schiaparelli to the conclusion that their rotation period is the same as their sidereal period of revolution around the sun. Another paper, by the editor, Mr. E. Walter Maunder, entitled "The Chief Nebular Line," deals with the character and position of the chief line seen in the spectrum of the nebulæ, and its probable origin. Beginning with Dr. Huggins's discovery, in 1864, of the character of nebular spectra, it is shown how he suggested that the brightest line was due to an unknown form of nitrogen. This view was widely taught until 1887, when Prof. Lockyer enunciated the principles of his meteoritic hypothesis, and testified to the coincidence of the line with the remnant of the brightest magnesium fluting at λ 5006·5—a statement combated by the later observations of Dr. and Mrs. Huggins. Mr. Keeler's observations of nebular spectra, made at the Lick Observatory with a Rowland grating having 14,438 lines to the inch, demonstrate that the nebular line may appear both *more* and *less* refrangible than the brightest edge of the magnesium fluting. This being so, Mr. Maunder concludes "that we do not know the position of the nebular line with sufficient accuracy to say positively that it does or does not accord with the magnesium fluting." The Journal is supplied free to members of the Association.

ELEMENTS AND EPHEMERIS OF ZONA'S COMET (*e* 1890).—A Royal Astronomical Society circular contains the following elements and ephemeris computed by Dr. Hind. The orbit depends upon an observation at Rome on November 16, one by Baron von Engelhardt on the 18th, and the Paris observations of the 21st.

Perihelion passage, 1890 August 8·43592 G.M.T.

		°	′	″	
Longitude of perihelion (π)	...	113	16	52·1	Appt.
,, ,, ascending node (☊)		85	25	2·7	Eq.
Inclination (i)	... ...	25	38	57·4	Nov. 20.

Perihelion distance, 2·0597 (Earth's mean distance = 1).
The motion of the comet is retrograde.

Ephemeris for Greenwich Midnight.

1890.		R.A. h. m. s.		Decl. ° ′		Distance in Terms of the Earth's Mean Distance from the Sun.
Dec. 18	...	2 42 0	...	+32 19·9	...	1·757
,, 20	...	2 35 4	...	31 53·6		
,, 22	...	2 28 38	...	31 27·4	...	1·844
,, 24	...	2 22 42	...	31 1·5		
,, 26	...	2 17 14	...	30 36·0	...	1·937

The comet is, therefore, still in Perseus, and moving towards Triangulum. The sidereal time at Greenwich at 10 p.m., on December 18 = 3h. 49m. 50s. The intensity of light on December 26 = 0·47, that on November 16, the date of discovery, being taken as unity.

CHEMICAL ACTION AND THE CONSERVATION OF ENERGY.

THE conservation of energy is accepted as a general principle without question, but its operation is often so disguised, especially in chemical changes, that it is not apparent to a superficial observer, and, as a consequence, it is too often treated as a dead letter.

This is largely due to the enunciations given by thermochemists, who attempt to draw an impossible distinction between chemical and physical changes, and imply that the latter are not subservient to the law of the conservation of energy. The accepted principles of dissociation, together with the recognition of the complex nature of liquid molecules, and of the existence of compounds in solution, gives us, however, the means of explaining *all* the thermal results of any action in accordance with the recognized principles of science (see Chem. Soc. Trans., 1889, 14).

Chemical affinity, or the potential energy possessed by atoms, becomes satisfied to a greater or less extent when these atoms

combine together, and a corresponding amount of kinetic energy must be developed. In all ordinary calorimetric operations this kinetic energy takes the form of heat. An "endothermic" compound is, therefore, an impossibility, if the term be used in the sense of a "body formed from its constituent *atoms* with absorption of heat."

The same principle which governs atomic combinations must also govern those complex reactions with which we generally deal. They occur in order to satisfy affinity—in order to convert potential into kinetic energy—and heat *must*, therefore, be liberated during them. If any absorption of heat does occur, it can only be due to some secondary decomposition, which has followed the primary heat-evolving action, owing to the alteration in conditions occasioned by this action. Dissociation affords a simple explanation of the occurrence of such secondary decompositions.

Just as a certain temperature must be attained before any particular combination can occur, so there is a certain temperature above which any particular compound cannot exist; but, owing to the molecules in a mass of fluid being at different temperatures, dissociation begins when the average temperature of the mass is below this point, and is not complete till the average temperature is considerably above it. The stable condition of a fluid at any temperature between these limits is such that there are $\frac{1}{x}$th of the total molecules dissociated: if this condition be disturbed by the removal of any of the dissociation products, other molecules will have to dissociate in order to reproduce it.

The effect of dissociation on the thermal value of any change may be illustrated by the case of carbon dioxide and carbon, which react at 600° to produce carbon protoxide with an absorption of 39,000 cal. At 600° carbon dioxide is partially dissociated, and consists of $xCO_2 + (1 - x)CO + (1 + x)\frac{1}{2}O_2$. With the free oxygen in this mixture the carbon can combine, *evolving* 29,000 cal.; and more of the carbon dioxide must then dissociate to reproduce the stable condition; this dissociation absorbs 68,000, leaving 39,000 cal. as the algebraic sum of the combination and consequent decomposition.

Nearly every series of reactions in which heat is absorbed can, I believe, be fully explained by one of the reagents being to start with in a state of partial dissociation,[1] and in the remaining few a similar explanation is obtained in the dissociation of the product of the reaction.

An exothermic reaction cannot possibly be impeded by the fact that its occurrence will *subsequently* involve a greater absorption of heat. The carbon cannot be supposed to refrain from combining with the free oxygen by the fact that its doing so will disturb the equilibrium of the mass and necessitate the decomposition of other CO_2 molecules. To imagine that it would do so, would be to endow the atoms with intelligence.

The converse fallacy is often held—that an endothermic reaction may occur if it forms part of a cycle of which the final result is an evolution of heat. This is obviously impossible. Unless we endow the molecules with prescience, we can no more imagine that they will react at first so as to increase their stock of potential energy (absorb heat) in order that this stock may subsequently be diminished by the interaction of the bodies first formed, than we can imagine that a stone will roll of its own accord a short way up a hill in order to have a long roll down on the opposite side.

A considerable amount of misconception also exists as to the influence of heat in effecting endothermic reactions. No amount of heating can make an endothermic reaction possible so long as it remains endothermic; but it is, of course, quite possible that a reaction, which would be endothermic at one temperature, may, owing to the relative magnitude of the heat capacities of the reagents, become exothermic at another; or that heating the reagents may induce dissociation, and that the new reagents thus introduced may render an action possible which was impossible with the original reagents.

Another not uncommon error is to imagine that an endothermic reaction may be brought about by the simultaneous occurrence of some independent reaction evolving heat. If the second reaction is really independent of the first, it cannot possibly have any influence on it; the heat liberated during it will be no more capable of rendering the endothermic reaction possible than heat supplied from any other source. Cases in which the contrary appears to take place may be explained either by the formation of new reagents during the exothermic action, or by the previous combination of the body; which reacts exothermically with that which could by itself react endothermically only.

[1] The amount of dissociation necessary is almost infinitesimal, and may often be too small to be recognized by other means.

Endothermic changes in which one or more of the reagents is a gas either do not occur directly, or, in the one or two cases where they do so, the thermal value of the reaction at the temperature of its occurrence is not known, or else the results may be easily explained in the same way as in the case of the reaction between carbon and carbon dioxide. The reactions which present serious difficulties are those where one or more of the reagents is liquid, such as (1) the dissolution of a solid in a liquid; (2) the dilution of a strong solution; (3) double decomposition between two substances in solution.

The heat absorbed when a solid salt is dissolved in a solvent—water, for example—must be attributed, not merely to the fusion, but also to the volatilization of the salt, and would amount to 5000 to 15,000 cal. per gram-molecule. To bring this about, some affinity must be brought into play capable of developing more than this amount of heat. Now, the satisfaction of the affinity of a large proportion of water for the salt molecule present develops less—the heat of dissolution is a negative quantity; but the water consists of aggregates of the fundamental molecules in a state of partial dissociation; and, from the fact that water will give off its fundamental molecules (*i.e.* has a vapour tension) at ordinary temperatures, we must conclude, I think, that this dissociation extends so far that some of the fundamental molecules themselves are present. To convert a mass of water at ordinary temperatures into its fundamental molecules (into gas), requires an absorption of 10,000 cal. per H_2O, so that those fundamental molecules must possess an amount of potential energy equivalent to 10,000 cal. more than that possessed by the average aggregate: they would, consequently, be capable of combining with a salt molecule and evolving 10,000 cal. more per H_2O than the average aggregate would, and they would be capable of effecting a combination which the average aggregate could not. The combination of one, or at most two, such fundamental molecules with a salt molecule would evolve more heat than that absorbed in the separation of the salt molecule from its fellows: we have, therefore, the conditions necessary for a possible reaction—a primary action *evolving* heat. The removal of the fundamental water molecules would necessitate the dissociation of other aggregates to supply their place, and these, in their turn, would combine with the salt hydrate present till the highest hydrate capable of existing under the circumstances was formed. The heat absorbed in the decomposition of the water aggregates is, however, nearly entirely counterbalanced by the recombination of the water molecules with each other after they have combined with the salt, for there is independent evidence to show that the water molecules present in a very complex hydrate are as much combined with each other as they are in the aggregates of pure water: thus, the net result obtained, when dissolution is complete, is simply the algebraic sum of two quantities: (1) the heat evolved in the combination of the salt with the water aggregates, (2) the heat absorbed in volatilizing the salt; and, according as the former or latter of these is the greater, so will the heat of dissolution be positive or negative; but the motive power, if I may use such a term, which produces these results is the potential energy possessed by the free water molecules, an energy which enables them to overcome the affinity of the salt molecules for each other, and produce a primary heat-evolving reaction.

The second case in which heat is absorbed by the dilution of a strong solution occurs as a part of the process of the dissolution of a solid salt, and is comprised in the above explanation.

Cases of double decomposition in which heat is absorbed would require too long an explanation for insertion here, and reference must be made to my former communication on this subject: it will suffice to say that they can in all cases be explained in accordance with the principle of there being a primary exothermic reaction, by recognizing the presence of several hydrates of various degrees of complexity, the less complex of these bringing about this primary reaction, and its removal necessitating as a consequence the endothermic decomposition of the higher hydrates to supply its place.

If the three conditions necessary to render a reaction possible exist—(1) a certain proximity of the reagents, (2) a certain affinity, *i.e.* power of developing heat by their reaction,

(3) a temperature within certain fixed limits—it follows that the reaction in question must occur; for, if it did not, the atoms would be in a state of strain inconsistent with stability; it would be as if a stone did not fall to the earth when there was nothing to prevent its falling. As a consequence of this, it follows that, in any complex system of atoms, where two or more different arrangements are independently possible, and where the various products remain within the sphere of action, and are capable of further interaction, then, those products, the formation of which is attended with the greater evolution of heat, will be formed to the exclusion of the others; or, if the two actions develop the same amount of heat, they will both occur to an equal extent.

I showed that the division of a base between two acids takes place entirely in accordance with this principle; wherever the salt formed remains undissociated in the liquid, the base is divided equally between the two acids, because the heat of neutralization of all such acids is the same, whereas, in other cases, where one of the salts formed is stable, and the other is in a state of partial dissociation, the undissociated salt is formed to the exclusion of the dissociated one; such small divergencies from this rule as are observed being due to the solutions examined being considerably stronger than they should have been, the heat of neutralization in such cases not exhibiting absolute constancy, and the dissociation of the dissociated salts being incomplete. This simple principle does away with the cumbrous hypothesis that each acid and base possesses a certain "avidity" or "affinity" peculiar to itself—an hypothesis which is, as I then pointed out, at variance with many facts of the case. A very striking confirmation of my views has been afforded by finding that the heat of neutralization of sulphuric acid in very weak solution is normal, thus verifying a prediction which I made on the strength of the above considerations.

I formerly held that the dissociation which explains endothermic reactions cannot be that of the product, but only that of the reagents; this I still think is true in cases where the product would dissociate into the same substances as the reagent would (if this latter could dissociate)—*e.g.* the hydrate of a salt dissociating into water and the elements composing the salt—but it is not true in other cases; for instance, a hydrate formed by dissolving a salt may dissociate into acid and base, and cause thereby an absorption of heat.

As with two possible reactions, where the products remain within the sphere of action, that which develops the most heat will occur to the exclusion of the other, so in two possible decompositions where the products remain within the sphere of action, that which absorbs least heat will occur to the exclusion of the other; and the question consequently arises, Why does a hydrated salt dissociate into acid and base, instead of into the anhydrous salt and water? The probable explanation is that in many cases the latter dissociation would be the more endothermic of the two; for the salt molecules in various contiguous hydrates are so far removed from the sphere of each other's attraction, that their attraction for each other would be practically *nil*, and the hydrate could only dissociate to form water and *free* salt molecules, absorbing in so doing a quantity of heat exceeding the observed heat of dissolution by an amount corresponding to the heat of fusion and volatilization of the salt, and exceeding in many cases the heat of neutralization itself. The presence of excess of water would moreover practically prevent the dissociation of the hydrate into water and the anhydrous salt.

A class of endothermic changes which present considerable difficulties are those which occur without the absorption of external energy in living organisms. We are at present so utterly ignorant of the nature of the reagents and products that it is hopeless to attempt any explanation of the *modus operandi* in these cases, but a suggestion made by Mr. Warrington at the recent meeting of the British Association with regard to the nitrifying organisms indicates the direction in which such an explanation may be obtained on the same principles as those given in the cases here discussed.

SPENCER U. PICKERING.

THE WORKING OF THE TECHNICAL INSTRUCTION ACT AND THE LOCAL TAXATION ACT.[1]

THE Secretaries of the National Association for the Promotion of Technical and Secondary Education, in reporting on the working of the Technical Instruction Act, have to congratulate the Association on the rapid progress which has been made during the last year in taking advantage of the benefits of the Act. This is partly to be ascribed to the grant of a small sum by the Science and Art Department to meet local effort, but chiefly to the allocation to County Councils of England and Wales, under the Local Taxation Act of this year, of a sum amounting in all to £743,000, with permission to use it for the benefit of education over and above any sum that may be raised by rate under the Technical Instruction Act.

Technical Instruction Act, 1889.—The number of local authorities in England and Wales which have taken advantage of the Technical Instruction Act has risen from *four* to *forty* since the date of the Manchester Conference a year ago. These districts are as follows:—Atherton, Aberystwith, Barnsley, Birmingham, Birkenhead, Blackburn, Burnley, Burslem, Bridgwater, Bingley, Bolton, Blaenau Festiniog, Cardiff, Coventry, Darwen, Guiseley, Keighley, Kidderminster, Manchester, Maidstone, New Mills, Macclesfield, Northampton, Nottingham, Oxford, Rochdale, Rotherham, Sheffield, Stockport, Salford, Shipley, Sherborne, Southport, Stalybridge, Worcester, Wakefield, Wrexham, Westmoreland, Widnes, and York. This list is probably not complete. In addition to the above, several of the Welsh County Councils have appointed their Intermediate Education Joint Committees to be Committees under the Technical Instruction Act.

Local Taxation Act, 1890.—In addition to these districts, a number of counties have taken action in the direction of utilizing the local taxation grant under the machinery of the Technical Instruction Act.

The following English counties have already resolved to set aside the whole or part of the money for education:—

Shropshire has set aside the whole of its share, amounting to £6543, for education, and the Technical Instruction Committee has presented a valuable report on the use of the money.

Somersetshire has set aside the whole of its share, about £11,000, for education, and is now engaged in considering applications.

The Technical Instruction Committee of Hertfordshire has recommended that the whole of the sum (£6429) be devoted to education.

Staffordshire has voted £7000 for education, and a scheme for its distribution is being framed.

Oxfordshire has set aside half of the grant, about £2000, for education, and Gloucestershire has also voted half the grant for the same purpose.

The North Riding of Yorkshire has voted £2000 for education.

Cheshire and Devonshire have determined to devote some part of the fund to education.

Leicestershire gives £300 to the Leicestershire Dairy Association, and Westmoreland gives £250 to existing schools, to be spent on apparatus.

The county borough of Croydon has voted the new fund to meet capital expenditure on technical instruction.

Besides these twelve districts (eleven counties and one county borough) which have definitely voted the whole or part of the new fund for education, the subject is now receiving careful consideration in a large number of districts, in many of which the new fund is practically certain to be applied, at least in part, to educational purposes.

Conspicuous among these districts is the county of Lancashire, which was the first to move in the matter, having appointed a Technical Instruction Committee in August last, which is just now reporting results of exhaustive inquiries as to the best means of assisting education from the new fund. Essex also has advertised for applications for a share of the money, and is now considering such applications with a view to assisting technical education. Committees have been appointed to consider the question in the following twenty counties:—Kent, Wiltshire, Cornwall, Berkshire, Northumberland, Peterborough, Southampton, Cumberland, Durham, East and West Sussex, Cambridgeshire, Worcestershire, Warwickshire, Dorset, West Riding, East Riding, Bedfordshire, and Herefordshire, and London. In London, however, the bulk of the grant for the present year has been used for the reduction of rates.

A large number of county boroughs are considering the desirability of using the new money for education. It is stated to be most probable that it will be so used in Nottingham, Salford, Blackburn, and Bradford. Worcester proposes to increase its grant under the Technical Instruction Act by £450, to be taken out of the new fund.

In addition to the above, the following county boroughs

[1] Secretaries' Report, read at the Conference on December 5.

among others, have the matter now under the consideration of a committee :—Barrow, Bootle, Burnley, Canterbury, Gateshead, Hull, Leeds, Norwich, Oxford, Southampton, and Wigan.

In Wales the County Councils, with at most one or two exceptions, are proposing to utilize all the new fund for the purposes of education. In most cases the money will be used under the Intermediate Education Act, but some Councils, as, for example, Cardiff, have resolved to divide the fund between intermediate and technical education.

The general results hitherto attained, so far as the information of the Association goes, which is probably incomplete, may be summarized as follows :—

English.

County Councils which have voted money to education under the new Act	11
County Councils which have appointed committees to consider the question	22
Total counties moving in the matter ...	33 out of 49

This result has been attained during the short period of four months since the passing of the new Act, during the first half of which time hardly any County Councils were sitting. The figures for county boroughs are as follows :—

English county boroughs working the Technical Instruction Act	17
County boroughs allotting the new fund for education, but not rating themselves ...	1
Other county boroughs considering the application of the new fund for education ...	9
Total county boroughs moving in the matter	27 out of 59
Other districts in England working the Technical Instruction Act	19

The general result gives a total of 47 local authorities as yet assisting technical education in England.

This is exclusive of the Welsh counties and county boroughs, which, broadly speaking, may be said to be devoting almost the whole of the new fund to the purposes of education.

The total number is likely to be very largely increased when it becomes generally known that there is every reason to believe that the grant, or at least so much as is applied to education, will be renewed in the future.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, December 8.—M. Duchartre in the chair.—Observations of minor planets made with the great meridian instrument of Paris Observatory, from Oct. 1, 1889, to March 31, 1890, communicated by Admiral Mouchez.—Experiments on the mechanical actions exercised on rocks by gases at high pressures and in very rapid movement, by M. Daubrée. The author applies the results recently arrived at (*Comptes rendus*, December 1) to the creation of volcanic ducts, and shows that it is probable that they are produced by mechanical actions much superior to the volcanic actions of which eruptions are the effects.—On the membrane of the lymphatic sac of the œsophagus of the frog, by M. Ranvier.—Proof that π cannot be the root of an algebraic equation having entire coefficients, by Prof. Sylvester. It is remarked that this proof should be substituted for the note by the same author, which unfortunately appeared in *Comptes rendus* of November 24. The latter note, which only dealt with a restricted case of the theorem enunciated in the text, is affected by inaccuracies which render it of no value.—A new method of studying the compressibility and the expansion of liquids and gases; results obtained with oxygen, hydrogen, nitrogen, and air, by M. E. H. Amagat. The experiments have been made between 0° and 200° C., and with pressures from 100 to 1000 atmospheres. The results obtained indicate that the coefficient of expansion of hydrogen at constant pressure diminishes regularly with increase of pressure. In the cases of oxygen, nitrogen, and air, the coefficient is at a maximum at the commencement, and this maximum corresponds to the pressure at which the product pv has the least value. The results obtained when the gases were reduced to constant volume show that $\frac{dp}{dt}$ is always greater between 0° and 100° than between 100° and 200°. The coefficients between 0° and 100°, at a pressure of 500 atmospheres, are 3·698, 3·085, 2·971, and 1·895, for oxygen, air, nitrogen, and hydrogen respectively. The values obtained at other pressures and temperatures follow a similar sequence.—Observations of Zona's comet, made with the great equatorial of Bordeaux Observatory, by MM. L. Picart and Courty. Observations for position were made on November 29 and 30.—On the observations of the transits of satellites of Jupiter and on the occultations of stars, by M. Ch. André.—On a transformation of motion, by M. Dautheville.—On the fluoride of allyl, by M. H. Meslans. Some of the physical and chemical properties of this compound are given. This ester is a colourless gas, possessing an alliaceous odour; it is formed from the iodide according to the equation $C_3H_5I + AgF = C_3H_5F + AgI$.—On some endothermic and exothermic reactions of organic alkalies, by M. Albert Colson.—On some derivatives of dimethylaniline, by M. Charles Lauth. A method of preparing tetramethylbenzidine is given, by which a yield of 40 per cent. may be obtained. Tetramethylbenzidine in hydrochloric acid solution is acted on at 45° by ferric chloride; fine green crystals of an unstable dye are produced, of which the formula is shown by analysis to be $C_{16}H_{21}ClN_2O$.—Contributions to the study of the nucleus of Spongidæ, by M. Joannes Chatin.—On the new class of jumping Acarina (*Nanorchestes amphibius*) from the coast of the British Channel, by M. Topsent and Dr. Trouessart.—On the age of sands and clays of the south-east, by MM. Ch. Depéret and V. Leenhardt.—Observations on extracts of meat, by M. Balland.

CONTENTS.